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ART. I.—THERAPEUTICAL RELATIONS OF INTERMITTENT FEVER.

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IT will doubtless be thought by some a little remarkable, that in the present state of medical knowledge there should appear in a leading medical journal, a communication upon so rudimental a topic as the one I have selected. This subject cannot be said to have been neglected, yet so complete has been medical certainty in reference to the treatment of this disease, since the antiperiodic powers of cinchona were discovered, that few physicians have thought it needful to seek for a more efficient plan of arresting the paroxysms of intermitting fever, than was devised at a very early period after the alkaloid of Peruvian bark was brought into use.

It is evident that the curative power of quinine will display itself in this disease, no matter in what form or quantity, or in what stage of the disease administered, yet it is equally evident, to me, that the specific relations of the drug to the malady are developed in their highest degree, only when administered under special conditions. During a period of ten years, with tolerably extensive opportunities of experimenting in the management of this disease, I have pursued every possible modification of the

quinine treatment, with the view of ascertaining as accurately as practicable, the conditions under which the highest remedial relations of the bark preparations are manifested. These observations have led to results highly satisfactory to myself, and in view of the probability that I may, in some degree, become instrumental in aiding, at least, younger members of the profession in escaping the perplexities, anxieties, and even disappointments which attended the first years of my own medical experience, I have thought I might confer a small service by committing some of my impressions to record.

Fourteen years from this time I made my professional debut at a small village situated on Barren river, in Warren county, and almost surrounded by an extensive body of fertile, alluvial bottom land. During the autumn of that year, intermitting fever was prevalent throughout Kentucky, and in the aforesaid bottom it was universal. After a brief, but spirited, combat with the disease in the persons of various of my new patrons, I was seized with a most relentless tertian myself. I was an intense sufferer during the ten or twelve days that followed, and well do I remember that my medical attendant watched over and administered to me most assiduously for well nigh this period before the arrestment of my "alternate day" paroxysms was accomplished. My case furnished a genuine specimen of tertian intermittent then as well as now. The cold stage was well marked, followed by prompt but rather violent reaction, and this stage terminating within from ten to twelve hours, in a gentle perspiration. Twenty-four hours, or more, of freedom from fever, with comparative comfort, intervening the paroxysms.

The treatment to which this case was subjected, consisted in active depletion—general blood-letting, emetics, mercurial purgatives, antimonials, &c., during the first three or four days, as preparatory, then the antiperiodic, quinine, in doses of one or two grains, repeated every second or third hour, during the apyrexial stage. This practice was not peculiar to the physician, whom I ought, perhaps, to remember with infinite gratitude, but it was the practice universally pursued by those around him, and the practice inculcated by those esteemed most trustworthy medical authorities, except in so far as these latter recommended a still

farther complication of measures. This treatment is founded in views not compatible with the true therapeutical relations of the disease, and I have, long since, repudiated it in the main. The medical world has been not a little addicted to the error of confounding with the disease itself what are only its necessary concomitants—its sequences. Since the days of Sydenham and Ramazzini the idea has been prevalent that serious obstructions, engorgements, inflammations, &c., must be removed as a fixed condition to a safe resort to the measures appropriate for arresting the paroxysm—due antiphlogistic preparation of the system. Engorgements, hypertrophies, inflammations, effusions, indurations, &c., have been made to assume a false attitude, as respects exact pathological relationships and as respects a rational therapeutical diagnosis.

No matter what may be the nature of the agent to which the production of the disease is due; no matter in what manner or through what avenue the reception of this disease-producing element into the system is effected, it is a settled conviction of my mind that the primary morbid impression is upon the nervous system, and that this impression *de novo*, constitutes the disease; that the various functional and structural lesions, giving rise to their appropriate morbid expressions, are consecutive events, and due, not to the operation of the primary agent, but owe their development to a secondary impulse, and that these manifestations are *ordinarily* multiplied and aggravated in proportion as the paroxysm is repeated. The disease is in its essential form functional, and every organic complication which its progress involves constitutes a complication and of necessity something more than genuine intermittent fever.

Innervation and circulation—the nervous and vascular functions—are so mutually dependent for integrity of action, that the slightest disturbance of either necessarily deranges the other, so that the primary impression upon the former ultimately becomes so intense, as to produce *remora* of the blood and consequent suspension of calorification—chill. This is the earliest *characteristic* phenomenon exhibited in the disease, but one which creates a necessity for the development of subsequent phenomena. These depressing effects are antagonised by the re-active efforts of the

organism—efforts that are salutary, only in so far as the restoration of heat and the circulation are concerned, while other functions remain suspended until farther, and still more salutary efforts are instituted for their restoration. This exaggerated state of matters subsides in a little while into a condition of things which displays nature's curative energies most strikingly in the various eleminative processes which are set up, tending to rid the economy of the primary morbific agent. Such efforts, unaided by art, are seldom completely adequate to the end, they prove sufficient only to effect such a diminution of the power of the vitiating agent, as to require the lapse of a given period before its impaired strength is again equal to the production of innervation so intensely perverted as to give rise to a new set of similar phenomena. Thus we have terminated the third or last stage of an intermittent paroxysm.

It is a question fairly admitting of discussion as to whether the duration of the period, constituting the interval, whether the anticipating or retarding tendency of the fits, be determined by the intrinsic virulence of the impressing agent, the result of the mere state of receptivity of the system at the period of exposure, or native impressibility of temperament. This inquiry involves questions to the solution of which I, at present, feel no disposition to address myself.

In my estimation it is in the third stage of an intermittent paroxysm, or the stage of declining excitement, alone that the natural tendencies to restoration are plainly exhibited. And the first and most important indication we have to fulfil, for the arrestment of the disease, is to aid and encourage these curative efforts already manifested by the economy. The office of remedies is not strictly to cure, but rather to support nature in her efforts to accomplish this end, and that material aid should be afforded by our remedial agents, they must be so administered as to stimulate the normal energies of the constitution just at the juncture when nature is struggling to counteract the morbific impulse; unless such co-operation is promoted, medical art is, in a great degree, exercised to no purpose, while nature's unaided efforts are seldom adequate to the complete and permanent arrestment of this peculiar malady. The first abnormal movement which we note, of a

characteristic kind, is the chill; this we have said is the direct result of some—not very well understood—morbid impression upon the nervous tissue. These are about the first links wrought in the chain of morbid actions ensuant, and if the severance of these be effected before the extending chain becomes riveted to other systems, the full consummation of therapeutical interference is attained. To accomplish this we have but to induce a new impression upon the nervous tissue, and as this is promptly done, will be limited and rendered less formidable, consecutive evils requiring subsequent correction. We cannot, just here, too earnestly urge the momentousness of exercising a thorough discrimination, as to what constitutes the disease *essentially*, and what are its ulterior consequences, that we may not bestow our therapeutics in vain endeavors to repair consequences, while the primary and absolute disease which gave rise to them is left to an undisturbed progress. The numerous lesions which have already been cited, while they are the common sequelæ of advanced intermittents, do no more constitute essential elements in the pathology of this fever than does herpes labialis, or the ptyalisms sometimes induced by the barbarous treatment employed for its cure.

Although the experience of several years has resulted in no material modification of the opinions embodied in this communication, yet, until the observations furnished by the present extraordinary season, have been subjected to a diligent clinical analysis have I felt sufficiently warranted in their correctness to present them to the profession. Within a brief space, embracing only a few weeks, I have dispensed medicine to several hundred subjects of this disease, many of them assuming most anomalous types, presenting themselves under every conceivable mask. Urticaria, ophthalmia, diarrhœa, mere stupor and persistent somnolency, have in instances constituted the more obvious pathological manifestations, the only identifying feature being periodicity, but which, upon careful analysis of their history and nature, together with the prompt and satisfactory results of the anti-periodic treatment, were certainly enough traceable to their family origin. Why it is that such freaks are displayed in the history of this disease I am under no pledge to explain; such cases are not frequent, but certain it is, they occasionally present themselves, and

I am almost persuaded that the obstinacy of treatment, now and then evinced by skin, bowel and eye diseases, result from the fact that treatment is instituted in the absence of a correct diagnosis. While from the assumed semiology, we have an apparent case of urticaria, ophthalmia, or diarrhœa, the real disease, at least in its therapeutical relations, is intermittent fever. This apparent incongruity in these cases, would of itself, constitute an interesting inquiry and I think, judiciously treated, a profitable one, but the more regularly developed type of the disease must occupy my attention for the present.

In regard to the leading principles which have governed my therapeutical procedure, in this disease. I have uniformly endeavored to thwart the recurrence of the paroxysm, and have rarely had to witness its repetition after the exhibition of a first prescription. Periodicity has been the leading diagnostic element, as well as a controlling characteristic in regard to treatment. The fixed purpose of acting in concert with the efforts of nature, in re-establishing the normal tone of the nervous system, and relieving the obstructed secretions, has obtained throughout. Complicating events have seldom been absent in cases not seen until the paroxysm had been several times repeated, such as functional disturbances, visceral engorgements, topical inflammations, &c., but as a primary consideration, these have been disregarded, except in so far as it was convenient to treat them *pari passu*, in arresting the paroxysm. My treatment has been less obnoxious to the charge of complication than routineism, though such practice is fully justified in view of the routine nature of the disease itself.

As before intimated, I regard the sweating stage or stage of declining excitement, as the only period of the disease in which the economy manifests a salutary antagonism. When this stage is fairly past, quiescense succeeds such efforts, until it again supervenes, and I usually embrace it as the "golden moment" for administering the following prescription. (We are not in every instance opportunely consulted, if too early or too late, it should be made a point to await its return.)

Hydrarg. Pillu, twelve grains; Sub. mu. Mercury, six grains; Ext. Gentian, ten grains; Sulph. Quinine, fifteen grains. Incor-

porate the mass and form into pills. Immediately upon the appearance of decided perspiration, or, if no sweating supervenes, so soon as the fever has abated to this point, administer two thirds of the pills, and the remaining third four hours afterwards. There may have been no rigor or chill, there may not have been cool sensations sufficient to arrest the patient's attention, this is of little moment; periodicity, as respects pyrexial symptoms, must constitute the criterion. This prescription accomplishes the double office, as nature's abettor, of arresting the paroxysm, and disgorging the viscera of their excretive accumulations, just at the moment when the efforts of the organism are most active in consummating the same object. In a very large proportion of recent cases this prescription will complete the treatment, but in cases of more advanced standing, the consequences of protracted congestions and violent reactions, often repeated, demand more special care. In the event of an already relaxed state of the bowels, this combination may require a small astringent addition, or, if perchance, cathartics have been employed already, it may be proper to exclude the mercurial entirely. A failure to thwart the ensuing stages, which I guarantee will rarely happen, will render it necessary to administer another ten grain dose of quinine, upon the advent of the third stage. The observance of these rules may not always be practicable, as in cases without a frank development, or what will still more embarrass absolute rules, those vicarious intermittents, if I may so denominate them, previously alluded to. The tendency of this disease to relapse is great, perhaps greater than any other, almost, in the whole nosological list; the most rigid propriety upon the part of the patient is requisite to avert it. If such an occurrence is apprehended I know of nothing more certainly calculated to avert it than the exhibition of a very gentle purgative, combined with ten grains of quinine, on, from the seventh to the tenth day of convalescence. If cases of this disease were at times promptly and judiciously treated, there need be but little concern bestowed upon secondary lesions, but unfortunately the safe rule is reversed, and we consequently encounter such sequelæ, aggravated and obstinate in the extreme. I this day witnessed, in the person of a previously robust negroman, a case of general dropsy consequent upon the protraction of

a neglected intermittent. These resulting troubles present themselves in such variety and extent as to form a wide field of investigation for the pathologico-therapeutical inquirer, and I am not forgetful either, that it has already been diligently and profitably cultivated. My present purpose will be more consistently conformed to by urging, at the expense of reiteration, the leading propositions which bear upon the disease in its essential and original form, in exclusion of events that owe their origin to its progress merely, and of necessity constitute adventitious features of the original. To sum up, I would insist upon the following as paramount propositions:—

1. Essential simplicity of the disease prior to the existence of secondary lesions, arising in its progress and directly the result of its continuance.

2. Importance of discriminating between the disease itself and its consequences merely, believing that these are often confounded to the prejudice of rational treatment.

3. Inutility of treatment, as preparatory to the arrestment of the paroxysm, as based upon the notion that inflammatory engorgements, &c., exert a material agency in the causation of the paroxysm, and that the removal of the former is a necessary pre-requisite to the safe arrestment of the latter.

4. Advantage and economy of responding to the efforts of the organism, at the most propitious juncture for the employment of remedial agents.

5. The third, or stage of declining febrile excitement, being the only one in which such efforts are conspicuously displayed.

It may be thought that in urging objections to some of the pathological doctrines hitherto promulgated upon this subject, and the recommendations of treatment deduced therefrom, I have assumed a bold position, but the fact that the teachings of modern pathologists simply consist in the endorsement of opinions handed down to them, by an earlier medical generation, affords some apology for such a position. Authors have mostly given an unnecessary and forbidding complexity to the subject. The disease is essentially a simple disease, and as a subject of practical discussion it is susceptible of being presented in an aspect correspondingly devoid of complication. Khowledge, of book and lec-

ture-room acquisition, constitutes, at a given period in the history of every medical man, the basis of his practical resources, and if this knowledge is to serve as his (only) guide in the discharge of his responsible duties in practice, rather than disconcert him at the bedside, simplicity should form the constant rule of those who assume the task of imparting it.

I trust these suggestions, offered as the result of bed-side impressions, may, in some degree, supply the absence of individual experience, in behalf of the young practitioner, for whom they are exclusively intended. I am persuaded that their adoption will insure to him, an easy and satisfactory career, so far as relates to the management of the simple disease, intermittent fever.

ART. II.—A CASE OF NEURALGIA OF THE FACE CURED BY EXSECTION OF PART OF THE NERVE. BY PROF. J. M. CARNOCHAN, OF THE NEW YORK MEDICAL COLLEGE.

REPORTED BY WM. P. CASSEDAY, M. D., OF LOUISVILLE, PROSECTOR AND CLINICAL ASSISTANT OF THE SAME.

THE following case of Neuralgia is one of great interest in reference to the cure of that disease by operation. As will be seen it was a case of a very stubborn nature, and it seemed almost useless to operate. This, however, seemed the only alternative, notwithstanding the seven different operations which had been performed. The same nerve had been divided twice by Dr. Jno. Watson, once by Dr. Willard Parker, three times by Dr. Mott, and once by Dr. Detmold.

The patient gave the following history of his case: Named J. C. Forbes; aged forty-seven, citizen of Hoboken, N. J.; married; of a nervo-bilious temperament; occupation, house carpenter.

He first noticed, in May, 1849, that when he drew his handkerchief across his upper lip and the end of his nose, a sharp, lancinating pain would shoot from near the middle of the lip, along the fossa at the junction of the nose and cheek on the left side, up to the inner angle of the eye. Also that when he touched his lip with his tongue, or attempted to swallow, he felt the same pain. These symptoms continued until autumn of the same year, without any change or any additional symptoms, when they wholly disappeared, and did not return until the next spring. In the spring of 1850 they returned in the same way, slight at first, but they increased much, and the patient thinking the disease might be caused by some bad teeth, and partly by the advice of a dentist, had every tooth extracted, except a small stump, on the right side of the lower jaw, which was overlooked. This gave no relief whatever. The disease increased and he suffered greatly with paroxysms over the whole left cheek. His sufferings were almost intolerable.

From this time until February 1852 the patient was under medical treatment, everything likely to do any good being tried, but in vain. In February, 1852, Dr. J. Watson dissected the entire cheek, from the superior maxilla, the dissection extending from the nose to the tuberosity of maxilla, and the malar bone, down to the alveolar border, and up to the orbit. It was made from the mouth with no external incision. This gave much relief until the next November when the paroxysms returned.

After this time until December he was treated occasionally with quinine. During the latter month he was again operated upon by the same surgeon, who made a V incision over the infra-orbital foramen, and divided the nerve coming out there. This again relieved him greatly, until autumn 1853, when the disease again appeared, and he was again put under medical treatment, but with no relief. Galvanism was tried at this time but ineffectually.

In January, 1854, Dr. Willard Parker, operated, making the same incisions as the last, and relieving the disease until August of the same year. The paroxysms then reappeared and continued until September, 1854, when Dr. Detmold operated, but with no good effect.

In October Dr. Mott divided the nerve by subcutaneous incision, and did the same in November. These operations afforded very slight relief, and on the 8th of January, 1855, Dr. Mott made a V incision, at the same spot at which Dr. Parker had made it, but more extended, and again divided the nerve. This afforded only partial relief. However the patient could attend to his business with little inconvenience.

In June, 1853, the disease again increased, and the patient went to the New York Hospital, and was under treatment there until August, when he applied to Dr. Carnochan, who treated him for a short time with Donovan's solution, aconite internally and externally, and an ointment of the extracts of belladonna, hyoscyamus and strammonium, but with no effect.

August 29th, the patient was no better, and it was decided to perform another operation.

August 31st, the patient thought he was a little better, but the pain still continued. He had been using veratrine since the last date.

This being the day appointed for the operation the patient was put under the influence of chloroform, when a V incision was made over the foramen, its base being turned toward the orbit, and the flap thus made dissected from the bone, and the dissection extended into the orbit, on its floor, for one inch. Proceeding cautiously, partly with the knife and partly tearing the tissues the foramen was found. It was difficult to isolate the nerve on account of the extensive hard cicatrix, the result of seven different operations at the same point. But the foramen being found and the nerve isolated, a small piece of the bone was removed from the edge of the orbit, and a part of its floor with a chisel and hammer. The canal was thus laid open half an inch, and a piece of the nerve in its entire diameter, was excised, being all that part exposed by the removal of the roof of the canal.

The operation was excessively painful as the patient could not be easily kept under the influence of the anæsthetic for any length of time. There seemed to be a peculiarly strong resistance to the operation of the chloroform. Although he was apparently insensible, when the nerve was touched he would start and in a very

few seconds be perfectly sensible and of course in great pain. Five ounces of chloroform were used during the operation.

Almost immediately after the excision he drank a glass of brandy and water with perfect ease, which for years before this was impossible, on account of the intolerable pain caused by it. He was sent to bed perfectly relieved of pain, and an anodyne administered.

September 1st, I saw the patient; I found him sitting up, in fine spirits, suffering no pain; he was awake much of the night before, but that was from excitement, not pain. He could eat without the least inconvenience, except that arising from the loss of his teeth.

September 5th, the patient presented himself at the office. He had no pain nor any constitutional symptoms. The dressings were removed and the surface of the wound was found to be freely suppurating. He could still eat without pain and was in as good condition as could be desired under the circumstances.

September 5th, was still in good condition. He complained of some soreness in the wound, which was covered with healthy granulations. His digestion was not good, furred tongue, etc.; he was ordered.

℞ Iodide Potass. ʒjss.
Tinct. Conium ʒij.
Water ʒviii. M. S.

Tablespoonful three times a day.

From this time the case progressed favorably. The wound is now entirely healed and the patient at work, having had no return of his disease. He complained at one time of pain along the track of the portio-dura, where it makes its exit from the stylo-mastoid foramen, which can be accounted for by the small recurrent filament which joins the portio-dura from the ganglion of Meckel.

He had an attack of intermittent fever Sept. 11th and 12th, and again about the 17th to 20th.

It is to be remembered that although the operations performed before this by Prof. Carnochan, relieved the patient, yet the relief was at no time so complete and perfect. The patient says he was

always in a worse condition immediately after those operations, but that after a certain time he recovered, and was much though not entirely relieved. But after this last the relief was immediate and entire. This seems to show that the better plan was to excise a portion of the nerve entirely rather than merely to divide it.

The case will be watched, and if there is any return of the disease it will be immediately reported.

New York, Oct. 1st, 1855.

ART. III.—ON THE MEANING OF THE TERM "SYMPTOM."

BY C. A. HUNT, M. D., OF URBANA, ILLINOIS.

THE editor of the *Peninsular Journal of Medicine* cites us to Webster's definition of 'symptoms,' and afterwards gives his own, both of which he seems to consider unexceptionable. Webster says, "a symptom is a perceptible change in the body or its functions which indicate disease." The editor says, "every intelligent physician knows that a symptom means a deviation from the normal condition of an organ or function, perceptible to the patient and to others, which indicates the presence of disease, and assists the judgement in forming an opinion of its seat, nature, etc, etc." I ask the privilege of differing on this point with both of these authorities, for I consider them essentially wrong in the whole.

"*A symptom, a perceptible change in the body or function.*" Suppose we discover in a certain part of the system an abscess, a tubercle, or a caries; would we not at once confess it to be a perceptible change in that part of the body as well as the functions of the part; yet who would contend that it was a symptom and not a disease itself? If we are called to a case of what is termed "idiopathic fever," this fever is the changed condition of parts and of functions by which it becomes entitled to the appellation

of disease. This perceptible change in the part and function is all we can recognize in a disease, for when the change ceases to be made in the body or its constituted parts, the functions of this body, it must then have reached the elements of the body and constitutes decay. But we see in the editor's definition precisely the same thing although it is more prolix and not so clear. The symptom according to him is "a deviation from the normal condition of an organ or function." Now the organ means the material structure, and the function, the office that it performs, and the deviation from the normal condition of the substance or the function is in all cases the pathological condition, or the disease in question. These three pathological states may exist, either fever, congestion or inflammation. This hyperemic condition incident to each is termed a disease, for they are express pathological conditions and are the perceptible changes of the organ or its functions, or of the body and its functions, yet who would call that condition a symptom?

Symptoms are the only evidence that we can possess of internal disease, by which alone we are enabled to arrive at the pathological condition. The first impress made by changing function or structure is upon some one or more parts of the economy, and must reach the brain by the afferent nerves, in the same manner that impressions from external nature do, to wit: by the sensory ganglia. These impressions in either case are resolved into sensations, the one normal, the other abnormal—the one received by organs endowed with nerves especially designed for that object, the other received by nerves whose office is the consequence of normal impressions from the interior economy and which become deranged by the infringement of foreign matter, hence any injurious impression is conveyed and received as such, and disbursed as such, and the sensation evolved is not normal function, but deranged feeling and function, and on the one hand is aggregated into pain by unequal distribution, and on the other is depraved into atrophy, etc., by the other excess. The manifestations of these impressions, made and disbursed are secondary to the cause in action which produced them, and become the only source of our knowledge of this internal change,—and are the symptoms proper; and in every case the manifestation comes secondary to

the changed and changing condition of the part and is a reflection. We can get no symptom of deranged action except the reflex action; for impressions are not made from the brain and spinal cord to the diseased part, but from the part affected to the nervous center; hence change of matter and phenomena must precede the evidences to the bystander as well as the patient. Even in case of an external wound, where, for instance, the skin is pinched, the contortions of the face and other motions are symptoms of pain, and the pain is a symptom of abnormal phenomena —but the part compressed is where the "perceptible change" is made in the body and its functions, and that is the spot which is diseased. This is precisely the *modus agendi* from the interior of the organism. Every abnormal impression proceeds from derangement made upon the matter or the force which acts through it, and is represented to the brain in the same unfriendly light and is reflected therefrom emerging into symptoms varying as the general tonicity is presented or not, and as the organs and functions vary in texture and office. In perfect health there is perfect satisfaction and no new phenomena are presented to the senses, but on the contrary unmixed harmony prevails, which, like a galvanic circle, is not disturbed, and gives no evidences of its latent force until obstructed by some foreign cause. When the disturbance begins the manifestation given through a responsive action is the first evidence we can have of the foreign obtrusion and this is the symptom. What then is the definition of a symptom? I answer, *the manifestation of abnormal phenomena.**

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*A symptom is any change perceptible to the senses, occurring in any organ or function, and connected with the exintence of disease.—*Chomel.*—D. W. Y.*

It is important for us to understand that the symptoms or signs of disease are never to be taken in the like sense with that in which the signs of external things are often regarded. The buoy which, floating in the river, marks its navigable tract; the bell which, by striking, denotes the lapse of time; the stone by the wayside, which tells us how far we have come, and how far we have yet to go; these are most important signs, and of indispensable service to us all, but they have no natural connexion with the things they are made to indicate. They are mere expedients of conventional meaning and use. Navigable rivers, and time and space, would still exist, though there were neither buoy, nor bell, nor milestone, in the universe.

There is nothing that we call the symptoms of a disease, which does not contain within itself much more than a mere sign. Heat, pain, redness, swelling, are called

In carrying out the illustration of symptomatology I will define also what I mean by disease of the abnormal phenomena of which I hold the symptoms to be the manifestation. A disease is the evidence of deranged matter or deranged force which acts through it. In a word the deranged matter or force, or both, develope the new phenomena, known as disease and the manifestation is the symptom. Are symptoms then, the only expression of disease? I answer they are—in saying which I mean internal disease, for those on the exterior can be known by the direct phenomena as presented to the sight without any accompanying symptoms. The symptoms of the disease then are the representatives of deranged action or phenomena, as words are representatives of thought or motives. The expressions of each are developed through mechanical force under increased or diminished muscular action; language through the voluntary muscles, and symptoms through both voluntary and involuntary, in proportion to the part affected. The muscles and all contractile fibres including the heart, capillaries, absorbents, excretory ducts, etc., become the media of symptomatic expression, while the muscles of the tongue, face, fauces, larynx, etc., are those used in language. Words are used to convey and are the evidences of impressions, normal or abnormal, those from the interior economy and those from external nature, while symptoms are evidences only and designed to convey abnormal impressions, having arisen out of disease or derangement.

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the signs of inflammation; but nature does not intend by them barely to intimate that inflammation exists; they are essentially connected with the processes she is carrying on.

Thus at early dawn we point to the first glimmering in the east, and call it a sign of the rising sun; but it is more—it is an emanation from his beams. We look at the cloud above our heads, and say it is a sign of rain; but it is the gathering of the waters themselves.

Concerning symptoms I would nevertheless remark (what is very important to be borne in mind), that they stand in different relations to the disease to which they belong. They may flow out of the disease, so as, in idea at least, to be separable from it; or they may be involved in the disease, so as to be identical with it. The difficult respiration, the cough, the sputa, the emaciation, the hectic, are the symptoms of phthisis, and are distinguishable from the disease itself. They are the signs of something beyond themselves, which we do not see—viz., tubercles of the lungs. But the symptoms which denote an intermittent fever are the same which constitute

A symptom then being the representative of the abnormal phenomena must always arise after the inception of the phenomena and end with their cessation. If disease has created, or is represented by a symptom, the evidence of its existence must cease with the cessation of the representative. When a disease has ceased to be represented, or ceased to develope a symptom, it must then be as harmless to the organism as before it developed one. But inasmuch as we have no other guide to the disease except through the symptoms, we must content ourselves with believing disease to have ended when it shows no more evidence of its existence either to us or to the patient. We, therefore, could not be justified in treating an organism for any disease when all the symptoms have ceased, for the exhibition of a remedy (if it have any effect at all) must at such a time, if not before, create symptoms of its own which in turn must be overcome by another remedy, or by the recuperative energies of life. Parts of the economy may at one time by excessive excitement eclipse symptoms in another part, yet when the violence of these shall have abated, those from the other part may become apparent, and in turn obscure the first by excess of action, yet the eclipse is no evidence of the cessation of the symptoms in either, but the total cessation of symptoms from each or both would be coincident with the disappearance of the new phenomena, to wit: the disease itself.

The commonly recieved views that disease (say fever,) holds an independent sovereignty in the economy, and gains its admission

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the disease. We have no idea of an intermittent apart from the rigor, the heat, and the perspiration. The same may be said of other fevers, and almost all diseases not organic, in which, if you seek to separate the symptoms from the disease, you must resort to theory for the purpose, and conceive an action of a certain kind prior to the actions which constitute the symptoms, and are productive of them.

There are then symptoms which, in the plain and intelligible sense, are signs and tokens of the disease that exists separately and distinctly from them; and there are symptoms which, however they may be spoken of as signs merely, are, nevertheless all that we know of the disease itself. The disease is the symptoms; and the symptoms are the disease.

True: but it can hardly be conceived that they are in reality the same. Yet it is better that they should be so regarded, than that we should go beyond our knowledge in attempting to distinguish them. Better to see in fevers only a certain combination of symptoms, than to run wild about a debility of the nerves, a spasm of the extreme vessels, or a peccant matter in the blood.

as an independent agent is very nearly deserving the epithet of superstition. On the contrary when a disease exists it is a phenomenon flowing constantly from deranged matter or deranged force, just as health or harmonious function is the invariable result of the normal condition of the matter and the force which developes life out of it. Life and health are but phenomena flowing from force acting through matter of a certain arrangement and certain integrity, and when that integrity and arrangement are violated, derangements aggregating into disease as certainly result, as health eventuates under the former circumstances, because in the economy the vital force propels perpetually phenomena which must be identified as normal or abnormal in the proportion that the relations of force to matter or matter to force are preserved or deranged. If my position is comprehended by the reader, he must see that disease is a phenomenon differing from the legitimate phenomena of normal force or matter called health, and that these phenomena are converted from one to the other by the derangement of the force or the matter through which the force acts. Now it follows that if health is known by sensation, by emotion, by feelings, and by impressions, the opposite condition, disease, is also known through the same media and the feature, the expression, the representation, which identifies this deranged phenomenon are the symptons, to-wit: The manifestation of deranged phenomena. When the indications of health fail to be presented the phenomena are termed disease. When the indications of organic life thus fail it is termed death, yet the force or the matter

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Again: I would remark that it is often most difficult to draw the line between what is disease, and what is symptom; and that the same conditions may in different cases become now one and now the other. A dropsy, or a hæmorrhage, are sometimes primarily and essentially the disease. Sometimes they are secondary, and incidental to the real disease, and are themselves only symptoms.

How impossible, then, must it be to give any definition of a symptom which shall be philosophically true, and at the same time satisfy every sense in which it is practically regarded!

From the imperfection of our knowledge the whole subject of semeiology is beset with philosophical difficulties; and no advantage will be gained from conducting our inquiry concerning it in a stricter method that its own nature will bear.

Whatever it is that bespeaks the presence of diseases, or denotes their nature or their seat, or moreover, whatever indicates the proper method of treating them, may be equally regarded in the character of a symptom.—*Latham.—D. W. Y.*

are not lost but only changed in relation. From the preservation of the force and matter in their integrity flows health; from their derangement flows disease; from their destruction flows death, and the expressions, and features, and representations of each termed symptoms, are marked and must exist with the existence of each and end with the termination of each.

Urbana, Ill. Oct., 1855.

Bibliographical Notices.

Art. IV.—*Yellow Fever, considered in its Historical, Pathological, Etiological, and Therapeutical Relations, including a sketch of the Disease as it has occurred in Philadelphia, from* 1699 *to* 1854, *with an examination of the connections between it and the fevers known under the same name in other parts of Temperate, as well as in Tropical, Regions.* By R. La Roche, M. D., Member of the American Philosophical Society; of the American Medical Association; Fellow of the College of Physicians of Philadelphia; Corresponding Member of the Imperial Academy of Medicine, and Foreign Associate of the Medical Society of Emulation, of Paris; of the Academies of Sciences of Turin, Copenhagen, Stockholm, Nancy, and New Orleans, of the Medical Societies of Naples, Marseilles, Lyons, etc. Philadelphia: Blanchard & Lea; 1855.

Dr. La Roche is known to have been for many years engaged in the preparation of a work on yellow fever. It has, at last, appeared, and never could it have made its advent on an occasion more opportune that the present. Yellow fever has within a short period become invested with a new and pervading interest. After

a long absence from cities where it once prevailed, it has recently appeared again, and its diffusion over our country has been wider, within a few years, than was ever known before. It has made its appearance in localities where it never existed, and invaded districts where it was thought it could never reach. Its mortality has been very great. This elaborate treatise of Dr. La Roche will, therefore, be hailed by the profession as a most seasonable contribution to our literature on the subject. His work, though treating more especially of the yellow fever in Philadelphia, aims at a more enlarged and important character—at presenting an *expose* of the whole subject, its symptomatology, anatomical characters, pathology, treatment, etiology, and the laws by which it is governed. The task in which the laborious author has been so long engaged has been nobly performed. The result of his pains-taking efforts is a work which reflects credit upon his learning, judgment and taste, and does honor to our country. It is a great work—great for its research, for its historical details, its statistics, its learning, and the spirit of philosophy with which it is instinct. In point of size it also belongs to the class of the great; it is comprised in two large octavo volumes of more than fourteen hundred pages. The reader must be curious about yellow fever who cannot find here every point discussed in which he can feel any interest.

Dr. La Roche prefaces his treatise with an account of the medical topography of Philadelphia, together with a history of yellow fever in that city. We pass over all that he says on the former subject, to give some extracts from his interesting history.

Yellow fever first appeared in Philadelphia in 1699, when scarcely a few hundred families from the mother country had clustered together there, the first settlement having been made seventeen years before. The mortality was proportionally as great as it has ever been in any subsequent visitation of the fever. No professional account of this earliest epidemic has been given. Dr. La Roche gleans a meagre, but instructive, history of it from the journals of the few travelers who visited the place, the manuscript correspondence of one or two of its citizens, and the pages of the early historians of the colonies. It broke out about the middle of June, in a very hot season, and was regarded by

the citizens as similar to the fever which scourged Barbadoes and other tropical countries. Mr. Norris, the great-grandfather of the present distinguished Dr. George W. Norris, of Philadelphia, wrote to a friend that the summer, about harvest time had been the hottest that he had ever felt, and added, that he "really thought that several died in the field with the violence of the heat." The number of deaths reported by the grave digger was 220 out of a population of 3,800, or about 1 in 17 of the inhabitants, a mortality that must have been frightful to the colonists.

"Well can we understand the dismay which this wide-spreading mortality created among the peaceful inhabitants of the rising city, as strongly depicted by Story. "Great," he says, "was the majesty and hand of the Lord; great was the fear that fell upon all flesh; I saw no lofty, airy countenance, nor heard any vain jesting to move men to laughter; nor witty repartee to raise mirth; nor extravagant feasting to excite the lusts and desires of the flesh above measure; but every face gathered paleness, and many hearts were humbled and countenances fallen and sunk, as such that waited every moment to be summoned to the bar, and numbered to the grave."

From 1699 to the year 1741 we hear nothing of yellow fever in Philadelphia, during which period it visited many southern ports in the Atlantic and five times prevailed at Charleston. Dr. Bard states that in the year 1802, a malignant fever broke out in New York, which, he says, was but little inferior to the plague, and which was probably yellow fever. It was reported to have been imported from "St. Thomas in a single bale of cotton."

Though Philadelphia maintained as free intercourse as before with the West Indies and the neighboring cities, she remained unscathed during the long period above mentioned, but in 1741, while prevailing in Jamaica, and other West India islands, the fever broke out with violence, and continued until checked by the advent of cold weather. The following extract of a letter written in 1741 by Mr. Peters, quoted by Dr. La Roche, will be read with interest, as containing valuable facts and as setting forth the views of the time in reference to the introduction of the fever.

"At the time of your brother's departure from this city, it was afflicted with a most malignant distemper imported in the spring by ships from Ireland. At first it crept along the wharves, affecting strangers, common sailors, and the low and poor part of the

people, and was easily managed by the physicians; but, as the dog-days advanced, having a long and dry season without gusts, the sickness increased and attacked the better sort of the inhabitants, of whom a good many died, though not in such numbers as is reported. About the 17th August, the physicians opened the body of a man who died in forty-eight hours from the first sensible attack, and found his bowels and stomach quite mortified. From the 17th August to 17th September, the distemper continued very violent, and baffled all the art of the doctors. I stayed a few days at New York after your brother went, and there, by accident, I lighted on Dr. Warren's treatise of the yellow fever at Barbadoes, a book well known in London, and finding his description answer in most points to the Philadelphia sickness, I dispatched it immediately to Dr. Grœme, who was much assisted by it; and, after the arrival of the book, the distemper came to be well understood, so as to yield soon, in very violent cases, to a free use of sudorifics—Dr. Warren's specific for the cure. One particular symptom attended the fever in Philadelphia which I suppose never appeared in Barbadoes, and that is a suppression of urine from the first attack. This was the case of poor ——; and when the urine was suppressed, there is no instance of any one's recovery. At present, we enjoy good health, and a fine season. According to the best accounts I am able to get, the number of the dead since the 1st of June amonnts to no more than 250."

Some mention is made of the appearance of the fever in 1744, but the testimony to that point is not clear, and it may be doubted whether it returned until the 1747, when, according to a letter of Dr. Franklin's, it continued to prevail till arrested by the accession of frost. In 1760 a few cases of the disease appeared, but it was not epidemic. Two years later it made its appearance in a malignant form, preceded by very hot weather, and, as usual, was arrested by frost.

"We have no precise information," says Dr. La Roche, "as to the mortality resulting from the disease on the occasion before us. Mr. Willing estimates the loss in the vicinity of the wharf just named at upwards of sixty; but this estimate must fall far short of the number of those who fell victims to the disease, for the late Dr. Rush, who, in 1762, was studying medicine with Dr. Redman, states—in a note-book kept at the time, and of which he gives an extract in his account of a subsequent epidemic—that the disease spread like a plague, carrying off daily, for some time, upwards of twenty persons. When, with this in view, we bear in mind that the number of cases in daily attendance must, judging from the expressions of Dr. Rush, and the statement of Dr. Redman, have been considerable; that the disease proved highly malignant, and

prevailed during more than three months, we may naturally infer that the mortality was very large—amounting, probably, to several hundred."

The histories of this epidemic left by those who witnessed its progress are brief and imperfect. Dr. Rush who was then a student of medicine, recorded some of its features in his note-book, and Dr. Redman has left a short description of it, from which there can be no doubt that the epidemic was yellow fever. Very different is the information attainable in regard to the visitations of 1793, '94 and '97. The disease, while devastating New York and Charleston, left Philadelphia unassailed for nearly thirty years. In the meantime, the city had grown to more than a hundred years old, and had acquired an able body of professional men. These have left behind them many valuable documents relative to the epidemic of 1793, which may be said to form an era in the history of yellow fever, since, as remarked by our author, on the observations made on that occasion, is in no small measure based the earliest knowledge obtained respecting the phenomena, character, origin, and mode of propagation of that disease. Dr. La Roche gives a dismal picture of the moral condition of the afflicted city gleaned from the histories of the epidemic, written by Rush, Volney, Deveze, Carey, Helmuth and Nassy.

"Many of those who remained being afraid to walk the streets, lest they might meet with individuals impregnated with the seeds of the pestilential infection, shut themselves up in their houses. Thousands, of all sexes and ages, resorted to the smoke of tobacco as a preventive against the contagion. Some chewed garlic, and wore it about their persons. Others, carrying their caution to a still greater length, provided themseves with new lancets, to be used on themselves or their families, in order to avoid thereby contamination from the instruments of physicians or bleeders. Many houses, as we are informed, were scarcely a moment in the day free from the smell of gunpowder, burned tobacco, nitre sprinkled with vinegar, etc. Churches were deserted, or wholly closed —as also the Exchange, City Library, and public offices—while the greater number of public papers were discontinued. Never were the rooms of Philadelphia oftener and more thoroughly purified, scoured, and whitewashed, than on this frightful occasion. Few went abroad without protecting themselves by the constant application to their olfactory nerves of handkerchiefs, or sponges, impregnated with vinegar or camphor. Some were found carrying pieces of tarred rope in their hands or pockets, or camphor

bags tied around their necks. Many never walked on the foot-path, but took to the middle of the street to avoid being infected in passing houses where death from the disease had occurred. Acquaintances and friends avoided each other in the street, and contented themselves with a cold nod. Shaking of hands fell into disuse, and cases of persons shrinking back with affright at even the offer of the hand are recorded. A craped hat, or any other token of mourning, was carefully shunned; and many persons valued themselves on the skill and address with which they gained the windward of those they met, and all uniformly and hastily shifted their course at the sight of a hearse. (*Carey.*) "The streets," says Dr. Rush, "everywhere discovered marks of the distress of the city. In walking for many hundred yards few persons were met, except such as were in quest of a physician or bleeder, or the men who buried the dead. The hearse alone kept up the remembrance of the noise of carriages or carts in the streets. Funeral processions were laid aside. A black man, leading or driving a hearse, with a corpse on a pair of chain-wheels, and now and then half a dozen relations or friends following at a distance from it, met the eye in most of the streets of the city, at every hour of the day; while the noise of the same wheels, passing slowly over the pavements, kept alive anguish and fear in the sick and well every hour of the night."

"It was impossible to find carriers of the dead, as a number of those who had at first performed that sad office were soon infected by the disease. It became necessary, in consequence, to resort to carriages for that purpose. The number of attendants at funerals was greatly diminished, and everybody retired at the approach of a hearse, and windows and doors were shut as they passed. The day, frequently, would not suffice to bury all the dead, because no assistance was to be had in digging the graves. Much of this was, therefore, done by night."

By a majority of the physicians the epidemic was ascribed to importation from the sickly ports of the West Indies. The College of Physicians gave a decision in approval of that doctrine; but they did not pretend to say from what part, or at what time, or in what manner it was introduced. "Public report had derived it from several islands, had chased it from ship to ship, and from shore to shore, and finally, conveyed it at different times into the city, alternately by dead and living bodies," and it was on such fluctuating reports of the people that the opinion of the College was based.

But the opinion was not universal among the physicians of Phila-

delphia. Dr. Redman, the President of the College, Dr. Foulke and Dr. Leib dissented from the report; Dr. Hutchinson, the inspector of sickly vessels, in an official communication, remarked that it did not appear to be an imported disease; and Dr. Rush united with these physicians in assigning a local origin to the disease.

"As regards the mode of propagation of the disease, a greater unanimity of sentiment existed at the outset of the epidemic, and for some time after. By the very large majority of the physicians and the public generally—even by those who advocated the opinion of local origin—it was thought to be transmitted by contact or intercourse with the infected; in other words, through means of contagion. But, even at that early period, physicians were found who rejected such a belief, and taught that the cause of the extension of the fever was to be sought in the diffusion, through the agency of the air, of morbid exhalations, themselves the offspring of organic decomposition. Gradually, the doctrine of non-contagion gained converts, until, finally, the physicians of Philadelphia became divided into two almost hostile parties. From this moment may be dated the origin of the interminable dispute about contagion and non-contagion, which has continued ever since to occupy the attention of the medical profession in this and other countries."

The yellow fever of the following year was less fatal in its character, and there were physicians in Philadelphia at the time who denied its existence. During the two succeeding years there were sporadic cases in the city, but the disease was not epidemic. It is interesting to remark, that in the first of these years the epidemic prevailed extensively in the city of New York, carrying off in a few weeks upwards of seven hundred, and also in the cities of Norfolk and Charleston; and yet these facts failed to open the eyes of a majority of medical men to the folly of looking to foreign ports for the origin of the pestilence. Dr. La Roche attributes the escape of Philadelphia from the epidemic, at that time, to the favorable character of the prevailing weather.

In 1796 came another epidemic yellow fever. The range of the thermometer during the warm season of that year, though not so high as in 1793, was above 80 deg. of Fahrenheit. The mortality was less than that of the year just mentioned, but, according to Dr. Rush, amounted to nearly one thousand, and according to Dr. Pascalis to thirteen hundred.

Dr. La Roche thus speaks of the epidemic of 1798:

EPIDEMICS OF 1798, 1799, 1802 AND 1803.

Severely as the city of Philadelphia had been visited in 1797, and especially in the memorable autumn of 1793, it pleased Provdence to put its inhabitants, in 1798, to a trial of perhaps still greater magnitude. The epidemic of that year, happily the last of the kind I shall have to record, was the most extensive, in point of malignity and mortality, of any that had yet occurred in this city, and may, in these respects, be placed on a parallel with even those by which portions of Spain, and our own city of New Orleans, have on several occasions been devastated. It afforded the last great manifestation of the epidemic constitution of the atmosphere—so far, at least, as this city is concerned—which had evidently prevailed during several years, and given rise to so much distress, and which, at the season under consideration, gave evidence of its existence and wide diffusion by the elicitment of the disease not only in Philadelphia, but also in Boston, New York, Portsmouth, (N. H.), Newport, (R. I.), New London, Wilmington, (D.), Charleston, in most of which it assumed a severe form. True it is, as we shall see, the disease broke out the next year, and has done so at subsequent periods; but on those occasions, it appeared in a less violent epidemic garb, or showed itself only sporadically.

The spring of 1798 assumed, at the outset, a promising aspect. There was plentiful rain, and vegetation commenced early. But a change took place about the middle of April, when a snow storm, succeeded by very cold weather, injured the sprouting leaves and blossoms. The intense heat of summer came on suddenly; the thermometer of Fahrenheit rising, on the 1st of May, as high as 84°. However, this spell was of short duration, and during that month and June, the weather, though warm, was variable, and at times attended with frost. In July, the heat became considerable, and continued so throughout, excepting towards the middle of the month, when the temperature suddenly lowered in consequence of a hail storm which occured at some distance from the city. Towards the close of the month, the heat again became extreme, the thermometer ranging from 88° to 92°, and rising as high at 96°. Throughout August and September, the weather was marked by high temperature. During the latter part of May, and in June, part of July, and throughout August and September, the weather was very dry; and, as a consequence of the drought, whole fields were burnt up by the sun, and the crops of hay seriously injured.

The mean temperature of the season was, for

	Morning.	Mid-day.		Morning.	Mid-day
June	66.47	76.83	September	59.50	72.43
July	67.61	79.77	October	48.26	60.61
August	73.90	83.23	November	29.53	42.06

It is well to remark, that the mean temperature of the first three months of that series—the most important in reference to the developement or diffusion of the cause of yellow fever—was not lower at mid-day than 79 deg. 94 min., an elevation greater by several degrees than that observed in other yellow fever seasons, but below that of 1793, when, as we have seen, it rose to 81 deg, 37 min. During the season, the thermometer rose forty-one times at mid-day (3 o'clock) above 80 deg.*

The localities where the fever made its appearance and prevailed, were in no better condition, in point of cleanliness, than those that had been the theatre of its ravages at former periods. The sinks and sewers were, as heretofore, filled with putrefying materials, and emitted the same effluvia. Condie and Folwell, though far from being friendly to the doctrine of local origin, call attention nevertheless, to the impure condition of the city. They speak of many vacancies on the banks of the river, covered with a thick bed of miry filth; of the wharves becoming filled up with impure materials from the adjoining streets, and emitting at low water, a very offensive stench. They specially refer to Pegg's Run, with its wide and miry bottom, and with its banks unimproved, and rendered still more injurious by being made a receptacle for the offals from slaughter houses, tan yards, etc.; of the offensive condition of privies in thickly inhabited places; of the entire want of these in Water street, and the offensive smell issuing from them everywhere during moist, calm, and sultry weather—particularly towards the close of summer. The Academy of Medicine, in like manner, called attention to the subject, dwelling particularly on the "putrid exhalations of alleys, gutters, and docks, and of the stagnating waters in the neighborhood of the city."†

The disease began this year early in August. In the July preceding, and especially during the very hot weather that occurred in that month, cholera morbus prevailed to a considerable extent among children, and occasioned great mortality. Dysentery, also was rife; and towards the middle and close of the month, several cases of bilious fever of a highly malignant grade occurred in the western part of the city; and, in general, proved fatal. Everything, indeed—continuity of high temperature, dryness of weather, accumulation of sources of morbid exhalations, and the nature of the prevailing diseases, as well as the appearance, about the same time, of the fever in other parts of the Union, seemed to presage a sickly season; yet it was not until the 7th of August that the disease was officially announced by the Board of Health, on information communicated by the College of Physicians. Assum-

* Caldwell, Fever of 1805, p. 17. Agreeable to the observations of Cadwallader Evans, the mean temperature of June was 77 deg., of July 82 deg., and August 86 deg. 5 min., giving an average, for the period of 81 deg. 8 min.—(Eclec. Repert., vii, p. 428.

† Condie and Folwell, p. 50; Currie, p. 12; Rush, Med. Inq., iv. 48.

ing rapidly a fearful character of malignity, and spreading far and wide, it prevailed from that period through the rest of the summer, and during the whole of autumn. From the 1st of October the fever gradually abated, probably from a deficiency of subjects; for many families having, in consequence of great diminution in the mortality, returned to their homes, the disease broke out with renewed violence, and continued to prevail until checked by the advent of frost. On the 1st of November its total extinction was officially announced by the Board of Health. (*Currie*, p. 136).

As in most other epidemics, the principal seat of the disease, on this occasion, was the river side and the adjoining streets. It began about Spruce and Walnut streets, and wharves; but after a short time, and before the close of August, spread in nearly every part of the city. About the same time it appeared, as it had done before, in the district of Southwark, and the village of Kensington. On this occasion, and this only, the disease penetrated into the jail, situate at the corner of Sixth and Walnut streets,* and even into the Pennsylvania Hospital, in Pine, between Eighth and Ninth streets.†

The pathognomonic symtoms of the fever were the same this season as they had been on former occasions. The disease appeared, however, to possess a greater malignancy of character, and presented some peculiarities of phenomena, a notice of which will find a more appropriate place in another part of this volume.

The mortality in this memorable epidemic was larger—not in the aggregate, but in proportion to the number of individuals attacked, or those who continued exposed to the infection—than it had been during the autumn of 1793. The whole number of deaths reported to the Board of Health from the beginning of August to the close of the epidemic, was 3,645. But it is supposed that the fatal cases that occurred in the country would, if fully ascertained, swell the number to four thousand. Of these reported deaths, 514 occurred in the City or Fever Hospital, and 3,131 in the city and liberties.

From the following table it would appear that the disease attained—as was the case in 1797—its highest intensity in September:—

August (from 8th)	626
September	2,004
October	943
From 1st to 5th November	72‡

When we take into consideration the state of the population at the time of this epidemic, this mortality must, as that which had occurred five years before, appear very large. The whole number

* Currie, pp. 79, 144; Condie and Folwell, p. 75.
† Additional Facts, p. 33.
‡ Condie and Folwell, pp. 105, 6, 7, 8.

of inhabitants in the city and liberties in 1800 amounted, as we have seen, to something more than 70,000; but by the historians of the epidemic, the number, in 1798, was not estimated at more than 60,000.* Assuming the latter as the probable amount, it would follow that there died, during three months, 1 in 16.48 of the entire population. This proportion, which, under all circumstances, would appear large, must cease, to be so considered when we learn the state of public mind from an early period after the outset of the fever, and the result it produced almost immediately on the movement of the population. So early as the 7th of August, at the recommendation of the College of Physicians, the Board of Health suggested to the inhabitants of the infected district the necessity of removing into more healthy parts. The Academy of Medicine, in an official communication published on the 8th, recommended the same measure, as also that means should be taken to prevent the contaminated parts from being visited by the citizens; that all ships, and putrid articles of commerce, should be removed from the wharves and stores of the city; that the docks, wharves, yards, and cellars should be cleansed; that the gutters should be washed daily; that a sufficient number of physicians should be appointed to take care of such of the poor as might be affected with the fever; that the citizens should be advised to avoid all the usual exciting causes of fever—intemperance, fatigue excessive heat, the night air, and all violent and debilitating passions of the mind; and in every case of indisposition, slight in appearance, to apply immediately for medical however aid.†

These recommendations, though proper and advisable, called in a forcible manner, the attention of the public to the existing danger. Terror soon seized upon all classes. The alarm became general, and emigration was carried to an extent far beyond what had occurred in preceeding times of the calamity. The public offices and the banks were removed to the adjoining villages, and even physicians, few in number it is true, announced

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* Condie and Folwell, p. 55.

Currie gives a different enumeration; from 1st of August to 3d of November, 3,446.

August	623
September	1,831
October	942
November	50
	3,446

He adds 60 after the 3d, making 3,506; and 300 in the country. (Currie, p. 129.)

Mease says 3,637:

August	626
September	2,004
October	943
November	64.—(p. 37.)

† Condie and Folwell, pp. 47, 48.

their intention of leaving the city.* From Condie and Folwell, we learn that the number of the inhabitants who fled was variously estimated at from three-fourths to five-sixths of the population. They feel disposed to view the first as probably the correct estimate, and lay down at 40,000 the number who sought refuge in the purer air of the neighboring towns, or of the country (*Ib.* p. 55); leaving a comparatively small number exposed to the infection and to share the loss occasioned by it.

Some idea may be formed of the malignancy of the fever from the fact of the immense proportion of deaths to the number of persons attacked. The number of cases reported to the Board of Health amounted to 4,718, including 898 received at the City Hospital. Now, if we bear in mind the deaths reported fell but little short of 3,700 (3,645), we arrive at results of an almost unprecedented character. The proportion of deaths to the attacks was as 1 to about 1.27. That of the mortality in the City Hospital was as 1 to 1.75—the cases being 898, and the deaths 514; while in the city and liberties, where the number of cases amounted to 3,820, and the deaths to 3,131, we have a proportion of 1 in 1.23. These reports commenced only on the 18th, and the admissions at the hospital on the 7th. The deaths, however, till the 7th were 53. We may suppose that, from the commencement of the epidemic to the 18th, there may have occurred 150, which, when added to 4,718, gives us 4,868, or 1 death to 1.34.

The mortality in the early part of the epidemic was less severe than it was subsequently; for, while the proportion of deaths to cases throughout was as one 1 to 1.297, that from the 9th August to the 19th September was as 1 to 1.34—the cases reported being 1,937 and the deaths 1,424. In the hospital, the cases amounted to 535, and the deaths to 276—giving a proportion of 1 in 1.2, or thereabout, instead of 1 in 1.75.†

WHOLE NUMBER OF CASES REPORTED TO THE BOARD OF HEALTH, AND THE RESULT THEREOF.

	Cases Reported.	Deaths.	Proportion.
August	827	626	1 in 1.32
Adding	150		1 in 1.56
	977		
September	2,969	2,004	1 in 1.43
October and November (from 1st to 5th no admissions)	922	1,015	1 in 0.908
	4,718	3,645	1 in 1.297
Adding the 150	150		
	4,868		1 in 1.34

* Condie and Folwell, p. 57.
† Currie, pp. 84, 85; Condie and Folwell, Appendix, p. xiv.

CITY HOSPITAL.

	Cases Reported.	Deaths	Proportion.
August	222	112	1 in 1.2
September	483	276	1 in 1.75
October	193	126	1 in 1.53
	898	514	1 in 1.75

The great mortality of this season, in proportion to the remaining population, finds a ready explanation, doubtless, in the greater diffusion of the poison, but, also, in the fact that all, or nearly all, the inhabitants in easy and comfortable circumstances had fled from the infected city, and that the subjects on whom the disease bore were mostly to be found among the lower classes, who, residing in confined, ill-ventilated, and filthy courts and alleys, and unable to seek the benefit of a purer atmosphere, were more exposed to the influence of the morbific cause. The emigration in 1793 did not exceed, as we have seen, one-third of the inhabitants. Hence, among those who remained, a larger portion were of the better classes, who are less prone than the others to be attacked with the disease, and, when so attacked, have it in a lighter form; whereas, in 1798, few of that privileged class felt disposed to confront the danger. As regards the greater mortality in proportion to the number attacked, it was due solely to the greater malignancy of the disease, which, owing to this circumstance, combined with the unfavorable conditions of the classes attacked—arising from the causes just mentioned, as well as bad nursing, etc.,—baffled all the efforts of art, and occasioned nearly as many victims as it did sufferers. Dr. Rush, in taking notice of this disproportion of deaths to the number attacked, attributes it to another cause. It "was owing," he says, "to the liberal and general use of the lancet in 1793, and to the publications in 1797 having excited general fears and prejudices against it in 1798. Such," he continued, "was the influence of these publications, that many persons who had recovered from this fever in the two former years, by the use of depleting remedies, deserted the physicians who had prescribed for them, and put themselves under the care of physicians of opposite modes of practice. Most of them died."

The sufferings to which the poor and destitute were exposed during this public calamity, from the apprehension entertained of contagion, the wide diffusion and fatal tendency of the disease attained, and the panic that had seized upon every class of the community, an extent unparalleled in the annals of this country. Trade was completely paralyzed, intercourse was almost suspended, the working people were without employment; and while their dearest affections were torn asunder by the constant inroads of the disease, starvation threatened to fill the bitter cup of their sufferings. The public prints of the day, as also the work of Condie and Folwell

—the faithful chroniclers of the epidemic—contain details calculated to make every human heart bleed. It is gratifying to discover in the history of the time, that those who fled from the city were more hospitably and humanely received and treated in places where they had sought shelter than had been the case formerly.* Experience had already shown, that no fear of contagious contamination was to be apprehended from the effect of emigration.

By some, the early cases were attributed to a contagious virus, which had remained dormant from the preceding autumn in the beds and bedding of individuals who had died of the disease. The greater number attributed, as usual, the fever to importation from the West Indies, and vessels were confidently pointed out—the ship Deborah, Capt. Yard, from Jeremie; the schooner Aurora, from Cape Nicholas Mole; the Ariel, from the same; the brig Mary, from Kingston, Jamaica, etc. Faithful to the views he had heretofore entertained and expressed relative to the origin of the fever, Dr. Rush rejected the idea of importation, and referred the calamity to the operation of local sources of infection. Many other physicians—and the Academy of Medicine as a body—adopted the same opinion, which was destined to become soon that of the large majority. While such was the sentiment of these in relation to the origin of the fever, the greater number among them, and all the advocates of importation, adhered to the doctrine of contagion, and affirmed that, however the disease might originate, it was communicated from the sick to the well. But the events of the preceding year had induced some to modify their views on the subject, to limit the possibility of contagion within very narrow bounds, or even to deny it altogether. Those of the present sickly season made new converts. Not a few traitors were now to be found in the camp; and it is from this period we must date the revolution in Dr. Rush's mind relative to the subject, which he frankly avowed a few years after."

The fever reappeared the year following, but in a mitigated form and with less disposition to spread. It broke out earlier in the season than it had done the previous year, and spread slowly during the course of July, declining towards the beginning of August, and soon after almost disappearing for a time, but to revive and spread more rapidly and extensively until arrested by a thick frost in October. The mortality was comparatively small; Dr. Mease gives it at a little over 1,000, but this grew in part out of the general flight of the citizens. As on former occasions, the College of Physicians assigned to the epidemic a foreign origin,

* Condie and Folwell, p. 55.

but by the Academy of Medicine, of which Dr. Rush was a leading member, it was held to be generated in the city by domestic morbific exhalations.

During the summer and autumn of the two years succeeding the fever did not appear in Philadelphia except in sporadic cases; but while this city was comparatively exempt from the pestilence, New York, Providence, (R.I.), Norfolk, Baltimore, and Charleston suffered by its visitations.

Very different was the result in the year following, 1802, when the yellow fever once more made its appearance among us in the epidemic form. But, on this, as on other occasions, it reigned in a less general manner, and proved a source of much less distress and mortality than it had at several former epochs. I cannot find that anything in the character of the weather or temperature of the spring or early summer months, could have led any one to anticipate a greater developement of the disease than had occurred in 1800 and 1801. The month of March, as we learn from Dr. Rush, was wet, cold, and stormy, with the exception of a few pleasant days. Frost was common in April and in May. The weather was so cool as to make fires agreeable to the last, the wind blowing chiefly during the time from the northeast; while in the month of June, the weather was cold, rainy, and hot in succession; and, during the first two weeks of July, there fell a considerable quantity of rain. As regards the degree of temperature during the summer, June presented, at 3 P. M., a mean of 75.7; July a mean of 78; and August a mean of 78; the result for the three months being 77.2.*

The fever was preceded by the scarlatina anginosa, which, together with the cynanche trachealis, was the principal disease during the winter months. It reigned almost paramount during May and June, and extended into the succeeding month.

While Philadelphia was thus once more visited by the malignant fever, other cities of the Union suffered in an equal and even greater degree. It prevailed extensively in Baltimore, and especially at Wilmington, Del., where its progress, from the outset to the close, was marked by the utmost severity. It also prevailed in Charleston, while sporadic cases of the disease occurred in New York, Boston, and Portsmouth, N. H. Here it made its appearance, in isolated cases, on or about the 4th of July; the disease, in these instances, being sufficiently well characterized to be recognized as such by Drs. Currie and Cathrall, (pp. 6. 7). Its epidemic career did not commence, however, before the 15th of July, when it broke out near the corner of Front and Vine streets. On the

* Cadwallader Evans, Eclectic Rep., vii. p. 428.

2d of August, it extended to other parts of the city, particularly Front and Water streets, near the drawbridge, as well as to Chesnut street, near the wharf. The disease continued to prevail in different parts of the city during the months of August and September. The number of the cases increased after a white frost. The fever declined after a black frost which occurred on the 28th of October, and ceased entirely early in November.

As may very readily be imagined, the epidemic diffusion of the yellow fever in Philadelphia was a source of considerable alarm to the inhabitants, who had passed through many severe ordeals of the kind. But this alarm, great as it might have been, was much increased by the course pursued by the Board of Health. On the 4th of August, it publicly announced that the disease had made considerable progress, and that the mortality had been very alarming since its reappearance; though every part of the city and liberties, as far as they could learn, excepting the neighborhoods adjacent to the place where the fever originally appeared, remained remarkably healthy, and entirely exempt from any disease marked with malignant or dangerous symptoms. Dreading, as was stated, the renewal of the melancholy scenes of former years, and finding it would be impracticable, with their limited powers, to prevent intercourse with the infected, they declared the fever to be contagious, and recommended to the citizens to withdraw into the country. The advice, says Dr. Rush, was followed with uncommon degrees of terror and precipitation; and the almost entire population poured into the country, where they remained until publicly invited to return by the Board of Health on the 5th of October.

The mortality from the fever amounted, according to one authority,* to 835, and was distributed as follows: August, 262; September, 284; and October, 289. By Drs. Currie and Cathrall, the number is differently estimated. According to these writers, the mortality from all diseases, from July to October, inclusive, amounted to 1,108; *i. e.*, July, 261; August, 262; September, 284; October, 301 (p. 17). The number reported as affected, during the epidemic, was 598; of which number 307 died. During the same period, there died of other diseases 505 persons; making, altogether, 1,069. (*Ib.* p. 17). In the hospital, 110 patients were received, and of these 58 died. Admitting these statements to be correct, it follows that the proportion of deaths to cases was as 1 to 1.68. In the hospital, it was 1 to 2, or fifty per cent. The greatest mortality, in proportion to the number of sick, occurred between the 15th and the 25th of October.

The succeding year was another epidemic, but less diffused and less fatal than many of its predecessors. It was preceded and

* Mease, op. cit., p. 38.

accompanied by remittent and intermittent complaints. About the latter part of July cases appeared, but a favorable change of weather taking place, the fever was stopped and the city remained free from it until the close of the month of August when it broke out afresh with more severity than before. It spread extensively in September, but early in October it had greatly declined, and on the 20th of that month the city received the joyful announcement of of the extinction of the disease. The proportional mortality was small, being, according to Dr. Rush, about five in a hundred in the practice of most physicians, and the absolute mortality was less than 200. By this time the doctrine of the local origin and non-contagious nature of the fever made considerable progress in the profession, though the public continued to adhere as pertinaciously as ever to the old and contrary opinion.

In 1805, 1820, and 1853 the city again suffered incursions of the epidemic, but each in a form less malignant than the preceding.

Such is an outline of Dr. La Roche's history of yellow fever in Philadelphia, and with it we must close, for the present, our notice of his work. In our next number we shall present an analysis of other portions of it.

ART. V.—*A Manual of Clinical Medicine, and Physiological Diagnosis.* By J. H. TANNER, M. D., Licentiate of the Royal College of Physicians; Physician to the Hospital for Women, etc., etc. To which is added *The Code of Ethics of the American Medical Association.* Philadelphia: Blanchard & Lea. 1855. 12 mo., pp. 252.

CHOMEL'S Elements of General Pathology, translated by Drs. OLIVER and MORLAND, and published by Ticknor & Co., in 1848, although calculated to be one of the most useful works ever issued found its way into the hands of but few practitioners or students

south of Mason and Dixon's line. How many north of this boundary profited by its publication we have no means of knowing. We believe, however, that a second edition has never been called for in this country notwithstanding the original has reached its fourth edition in Paris.

On some account we regard Chomel's, or a similar work as indispensable to the student, and of no mean importance to the physician. We believe if it had been more generally read that the heavy harangues or mumbling prelections ridiculously called clinical lectures would often have assumed another and a better form, and clinical medicine have in every instance been rendered far more attractive both to those who labor to teach and those, less fortunate, who find it a labor to learn.

The work whose title forms the caption of this article is constructed measurably after the model of that of Chomel. It is, however, a manual and we are from principle averse to all manuals, compends, hand-books and remembrancers whatever. Yet, in view of the very limited circulation already referred to of Chomel's Elements, and the great hiatus that confessedly exists in this department of medical literature, we think Tanner's Clinical Medicine a well timed publication, and cheerfully recommend it to our readers.

Art. VII.—*A Manual of Pathological Anatomy.* By Carl Rokitansky, M. D., Curator of the Imperial Pathological Museum, and Professor at the University of Vienna, etc. Translated from the last German edition by Wm. Edward Swaine, M. D., Edward Sieveking, M. D., Charles Hewitt Moore, Geo. E. Day, M. D., F. R. S. Four volumes in two. Philadelphia: Blanchard & Lea. 1855. 8vo., pp. 586.

The long promised work of Dr. Carl Rokistansky is at length within reach of the American practitioner. We at last have a Pathological Anatomy in which things are described as they were

actually seen in the dead-house by one who was unfettered by preconceived notions or prejudiced views as to the disease of the individual examined.

"Rokitansky," as Mr. Wilde justly remarks, "differs from all other pathologists, in not engaging in the study or treatment of disease during life; he is not a practical physician, and seldom sees one of the many hundreds of cases, whose bodies he dissects."

The present work is based upon an experience running through a long series of years of a man who is represented on all hands to possess acute judgment, untiring industry and a spirit of candid research. There have been made under his eyes more than 30,000 cadaveric sections. Records of every case, taken down at his dictation, are kept, and all interesting specimens are preserved for the Pathological Museum. The facts observed and the conclusions deduced from them are embodied in the beautiful book before us.

"Whilst engaged," he says, "in working out the design of this Pathological Anatomy, I have throughout endeavored to act the part of a clinical teacher; and I believe that, in so doing, I have apprehended the requirements of our day, and usefully disposed of the colossal materials within my reach.

"The same self-reliance that characterised the commencement of my pathologico-anatomical studies, has stood by me whilst engaged in observing and interpreting the facts of which said materials are composed; for, each individual discovery encouraged me more and more to pin my faith upon nature alone. Still I have never failed to watch and to appreciate the achievements of other men.

"The present work will, at any rate, tend to show how thorough is my conviction that Pathological Anatomy must constitute the groundwork, not alone of all medical knowledge, but also of all medical treatment, nay, that it embraces all that medicine has to offer of positive knowledge, or at least of what is fundamental to it. Its domain will here, however, be found more extended, and more nearly approximated to the confines of Pathological Chemistry than has generally been the case in pathologico-anotomical writings."

The translation was executed by four gentlemen, each well qualified for the portion entrusted to him, and was published in London under the auspices of the Sydenham Society—a guarantee of its fidelity.

By presenting this "great store-house of pathological knowledge, in a convenient and accessible form," Messrs Blanchard & Lea have laid the profession in the United States under renewed obligations to them. The work is, in every respect, a first class publication.

D. W. Y.

Art. VII.—*A Practical Treatise on the Diseases of the Eye.* By Wm. Mackenzie, M. D., Surgeon Oculist in Scotland in ordinary to Her Majesty; Lecturer on the Eye in the University of Glasgow, and one of the Surgeons to the Glasgow Eye Infirmary. To which is prefixed an *Anatomical Introduction explanatory of a Horizontal section of the Human Eye-ball.* By Thomas Wharton Jones, Professor of Opthalmic Medicine and Surgery in University College, London, and Opthalmic Surgeon to the Hospital. With one hundred and seventy-five illustrations. From the fourth revised and enlarged London edition. With *Notes and Additions.* By Addwell Hewson, M. D., one of the Surgeons to Wills Hospital for Diseases of the Eye; Lecturer on Surgery in the Philadelphia Association for Medical Instruction, etc., etc. Philadelphia: Blanchard & Lea. 1855. 8vo., pp. 1027.

First Dr. Mackenzie wrote a large book on the Eye and published it in Glasgow. Then Dr. J. Wharton Jones prefixed an Anatomical Introduction explanatory of a horizontal section of the Human Eye-ball to a subsequent issue, and lastly Dr. Addinell Hewson, of Philadelphia, has annotated the most recent edition, and the work has been published in superb style by Blanchard &

Lea. It is a massive volume and its size has been increased about ten per cent. by the labors of the London and Philadelphia editors. It may be safely said to take the first rank among all publications on the diseases of which it treats. LAWRENCE on the Eye was the great book on that subject until MACKENZIE revised and enlarged his work which must now be regarded as *the* work on opthalmology.

A good deal has been said about the prevailing custom among the Philadelphia faculty of annotating English republications, and we think they have been very justly censured for so doing. Yet it occurs to us Dr. HEWSON is no more to blame than many older men who set him the example, and yet we do not remember that any of them were called to account for their misdeeds in this respect. Dr. HEWSON's additions to the work before us are quite as respectable as J. WHARTON JONES's are. They are a good deal more so in our opinion than Dr. KEATING's were to the last issue of RAMSBOTHAM's Process of Parturition, and, indeed, are quite equal to the labors of most of the editors whose names figure on the backs of so many of the works of our transatlantic brethren.

The sentiment of the profession is, however, opposed to the custom referred to, and Dr. HEWSON has happened to appear at a time when this sentiment was beginning to be expressed, and has come in for rather a large share of censure.

The editor's additions to MACKENZIE are brief, but many of them are of practical value. The first consists of a short account of the Opthalmoscope, and as we have been frequently asked and written to concerning this instrument and its uses, we copy entire the description of it given by Dr. HEWSON:

"To Prof. Helmholz, of Kœnigsberg, is due the credit of first devising an instrument, by which a sufficient amount of light can be thrown into the eye to enable us to see clearly its inner structures. His *eye speculum* consists, essentially, of a tube, with one end bevelled to an angle of 50° to its axis, on which is fastened the reflector of four parallel and highly polished slips of glass; the other end is cut square to the axis, and has adapted to it an eyepiece, containing a biconcave lens and diaphragm.

"An accident suggested this invention to Helmholtz. His friend Von Erlach, who wore spectacles, observed one day, whilst conversing with an acquaintance, that the eye of the latter became illuminated when the rays of the light from a neighboring window

were reflected by his glasses into this person's eye—hence the probable reason of Helmholz using plate glass as the reflector in his ophthalmoscope.

"Since Helmholz's instrument was first made known in 1851, Coccius Meyerstein, Follin Epkens, and many others, have invented instruments but little different from it in principle. In some a plain mirror, with a hole in it, has been substituted for the reflector of plate glass; in others, a biconvex lens has been added to condense the light as it falls on the reflector; in others, the same means has been employed when either the eye of the observer or that of the patient was short-sighted, to converge the rays as they enter or pass from the latter.

"The simplest form of eye speculum yet proposed, is that described and claimed as original by Anagnostakis. Although, on the authority of an informant of Mr. Dixon, we learn that Prof. Graeff showed this very instrument, or one precisely like it, to Dr. Anagnostakis, when he was in Berlin, in 1853, some time before the publication of his paper on this subject. It consists of a small concave mirror, about 2 inches in diameter, with a focal distance of four and a half inches. In the center of this mirror there is a small hole, of a quarter of an inch in diameter. This mirror, mounted on a handle, and protected on its plated surface by a sheet of blackened copper, constitutes the apparatus described by Anagnostakis, but except in persons who are short sighted, a distinct view of the deep seated structures of the eye cannot be obtained by it, without the aid of a biconvex lens of two and a half inch focus.

"Ruete's Ophthalmoscope consists of a mirror analogous to the one just described, but of ten inch focus, and a biconvex lens, placed before the patient's eye, to increase the convergence of the rays reflected from the mirror.

"Mr. Dixon has proposed to fix the mirror into a spectacle frame, to be worn by the observer, so that both his hands may be free, and enabled, without assistance, to command the movements of the patient's lids with one hand, whilst with the other he can use the magnifying glass. He also suggests the use of mirrors of a focus corresponding to the distance of distinct vision of the observer.

"We have, on these suggestions of Mr. Dixon, made for ourselves an *extempore* instrument, at a very trifling cost, by plating with tin-foil and mercury one side of a common bi-convex lens, having selected for the purpose a lens of such convexity as would, when plated, produce a mirror of a focus to suit our own eye. This mirror we covered with dark paper through which and the plating we made a hole of about the fifth of an inch in diameter, and then mounted the mirror in one side of a common iron spec-

tacle frame, having placed in the other a plane glass, so that the instrument would balance itself on the nose.

"The original instrument of Helmholz has not proved as efficient a one as is desirable in examining the retina; its illumination is not sufficient to give a clear view of the parts it is intended to expose. This objection cannot, however, be urged against some of the improved forms of the instrument, particularly those of Jäger and Meyerstein; but these, in common with many others, are so complicated in their construction, that we give a decided preference for the simple mirror and lens over all of them.

"*The mode of using the Instrument.*—All examinations with the eye speculum require to be conducted in a dark room, and it is also necessary that the pupil of the eye to be examined should be well dilated by the solution of atropia. A steady-burning, broad-flamed lamp will be found to be the best means of illuminating the reflector. Some recommend that the light should be placed to one side of the patient's face, and on the same plane with his and the observer's eye. This may undoubtedly be the best position for it in some instruments of a fixed angle of reflection, and may be even essential for their proper adjustment, but is not necessary where the simple concave mirror is employed. In using this we would decidedly prefer the lamp placed behind the patient's head, and so elevated that its rays will just clear his forehead to reach the observer's eye; for no matter where the light may be, we must, to throw the reflected rays into the patient's eye, compensate for the obliquity of the rays incident on the mirror, by giving a corresponding obliquity to the position of this reflecting surface. Hence, when the light is in the position we advocate, by elevating ourselves above the horizontal plane of the patient's eye, we will approach nearer the horizontal plane of the lamp, and thus diminish the angle of the incident rays, and this position will require much less obliquity of the reflector to illuminate the eye to be observed than is necessary where the light is at the side.

"The next step after arranging the position of the patient and the source of illumination, is to throw the reflection on the eye to be explored. This is done by turning the reflecting surface to the light, and then gradually changing its position until its reflection appears on the eye to be observed.

"The observer is now to apply his eye close to the orifice of the speculum (if he has not been looking through it before), and watching carefully the reflection, he is to move his head with the instrument back or forward, until the reflecting light appears about the size of the reflecting surface. The patient is then to be directed to keep his eye slightly inverted, perfectly steady, and wide open; its interior will then present a reddish hue, but none of the deep-seated structures will probably be recognizable. It is evident,

then, that the proper focal adjustment has not yet been obtained. By slightly moving the head and instrument, or by interposing a biconvex lens, this will be rectified. We prefer introducing the lens here, as it has afterwards to be used. If this does not make apparent the deep-seated structures, the head and reflector are not yet in their proper position, and must be moved nearer to, or further from, the patient's eye, until the observer can distinctly detect red vessels on the yellowish red ground of the retina and choroid. If the patient's eye is inverted, as we said it should be when the lens and speculum are properly adjusted, the light will, in all probability, fall on a brilliant white circle which is the entrance of the optic nerve, and from the center of this circle there will be readily seen two larger vessels, an artery and vein, passing up over the surface of the retina, and a similar set passing downwards over the same structure. This white circle has a diameter of one-fourth of an inch, and the contrast between it and the bright choroid shining through the retina surrounding it, is very striking. It is important that the whereabouts of this entrance of the optic nerve should be sought for at the first, as it is a valuable landmark, and its detection affords great facility in investigating the other parts; for here the illumination is brighter, being on a white surface, and the blood vessels are larger and more readily detected. Consequently, the observer becoming familiar with their appearance, will have much less difficulty in detecting them in situations not so favorable for their observation. Should the patient's eye be too much or not at all inverted, the light will fall on the surface of the retina, and the appearance presented will be that of a yellowish-red surface marked with streaks of darker red. Those dark streaks are the larger blood vessels, and by following them in the direction in which they increase in diameter, in other words, towards their origin, the observer will readily reach this landmark of the entrance of the optic nerve. Some have spoken of the facility with which they could, with the ophthalmoscope, see the pulsation in the larger arteries of the retina. The ability to detect such a phenomenon seems to us, however, rather doubtful. We believe the only mode of distinguishing the artery from the vein is by the darker color of the latter. To follow up one of these vessels in order to reach the entrance of the optic nerve, it is much better that the observer should move the instrument, the lens, and his own eye, rather than keeping these fixed to direct the patient to change the position of the eye under observation, for the latter cannot appreciate the character or exact amount of motion required to throw the light on the point sought after, and in attempting to do so, he will not unfrequently disturb completely the whole adjustment. Sometimes it will be only necessary to change the position of the convex lens to bring distinctly into view the parts we wish to observe. We need but re-

mind the beginner of the fact, that the lens, from its peculiar property, must be moved in the opposite direction to that in which the part is, which he desires to bring into proper focus, precisely as the microscopist has to move an object which he wishes to trace under the field of his instrument.

"The vessels coming out from the optic center were stated to consist of two pairs. One, an artery and vein, passing upwards in a more or less vertical direction; the other similar in character, but passing downwards. This is the course they generally take; they will sometimes be found to vary. It is exceedingly difficult to trace them very far, from the fact that the reddened surface over which they run makes it impossible to distinguish their minuter branches; and the iris completely intercepts the view of their final distribution.

"The bottom of a healthy eye presents, as was just observed, a yellowish-red color; this color is brighter in the immediate vicinity of the optic center than on the periphery of the retina. The tint will also be found to vary in different eyes—being lighter (more yellowish) in those who are fair than in those who are dark and florid; showing that the intensity and character of the color are influenced by the pigment layer of the choroid.

"Close to the inner side or the entrance of the optic nerve, the color will be observed to be darker at one point. This Helmholtz attributes to the shadow of the semilunar fold of the retina.

"When the patient looks directly at the eye of the observer, and thus brings the axis of the two eyes in the same line, the yellow spot of Soemmerring will come into view; the retina here is of a grayish yellow color, entirely free from any admixture of red; and has no blood vessels on its surface.

"As regards the value of the ophthalmoscope in the diagnosis of deep-seated disease of the eye, it might justly be supposed, from what has been said of this instrument, that it would not only be indispensable in the investigation of all such diseases, but that with it there should be no difficulty in detecting the slightest pathological change in any of these structures, and in determining the exact character of the disease present, without any regard to its subjective symptoms. A great deal more, however, has been expected of, and claimed for the instrument, than it is capable of accomplishing, in the present state of its construction.

"In the first place, the great concentration of light which it produces in the eye, renders its employment highly injurious, even for a few moments of time, in the incipient stages of disease. In cases where it can be endured, its employment for any length of time sufficient to detect all the changes which have taken place, produces an excited and unnatural condition of the structures which are the subject of investigation, and might readily lead the observer astray in his diagnosis.

"In cases of more confirmed disease, it might not give rise to such annoyances and serious consequesces; but would the investigation be of any value in such cases? Are they not generally incurable? Granting such to be the case, here will be found the great value of the instrument; for it, and it alone, will often enable us to set aside, as Mr. Dixon justly observes, 'as positively hopeless, a large number of cases formerly termed *amaurotic*, or *nervous*, which were assumed to be still curable, because their real nature could not be demonstrated.'

"'We now know,' he says, 'that total disintegration of the vitreous body, detachment of the retina from its connection with the choroid, and other equally hopeless conditions of structures essential to vision, may exist without any alterations being produced in the outward appearance of the eye. In enabling us, therefore, to appreciate these conditions, the ophthalmoscope has proved of immense value.'"

Art. VI.—*Medical Lexicon of Modern Terminology;* being a complete Vocabulary of Definitions, including all the Technical Terms employed by Writers and Teachers of Medical Science at the present day, and comprising several hundreds of words not found in any other Dictionary. Third Edition. By D. Meredith Reese, M. D., LL. D., Resident Physician of Bellevue Hospital, N. Y., etc., etc. New York: Samuel S. & Wm. Wood, 261 Pearl street. 1855. 12mo., pp. 233.

As stated in the preface, this little Lexicon "is simply a vocabulary of definitions," compressed in the smallest possible space and made of the briefest possible character consistent with perspicuity. It contains no reference to the etymology of terms and omits many obsolete technicalities and ancient terminological nomenclatures. But it presents, in the highest state of condensation nearly every term retained by modern authors or employed by most medical teachers. It is small; it is neat; it is handy; it is well printed and neatly bound. D. W. Y.

Editorial Department.

A NEW WORK ON DISEASES OF THE CHEST.

Prof. Flint, as we have stated in a former number, is engaged in preparing for the press an elaborate work on Diseases of the Chest. We learn that the work is far advanced, a large portion of it having been printed, and that it will be completed in a few months. Prof. Flint's time having been so much engrossed by this undertaking, he has been obliged to suspend, temporarily, the publication of his Lectures on Diseases of the Skin. His work being now nearly off his hands, he will soon have leisure to return to his Lectures.

LOUISVILLE MEDICAL CLUB.

We regret not to have been able to give in our present issue the Proceedings of the Louisville Medical Club for October. Nothing that we publish, we believe, is read with more interest than the proceedings of this Club, and in future we hope to be able to present to our readers a monthly summary of its transactions.

INTRODUCTORY LECTURES.

The season for Introductory Lectures has returned, and these pleasing annuals are pouring in upon us. We hope to find leisure to remark upon the merits of some of them during the winter. Among those which lie before us we cannot help speaking of the excellence of Prof. Dalton's, delivered in the New York College of Physicians and Surgeons.

NEW BOOKS.

We have received since the issue of our last number the following works, which will be noticed at greater length in a subsequent number: Carpenter's Human Physiology; Bedford's Obstetrical Clinique; Dickson's Elements of Medicine; Smith on Leucorrhea.

MISCELLANEA.

Editorial Changes.—Dr. H. D. Bulkley has resumed the editorship of the New York Medical Times.

Female Doctors.—Helen M. Gassett, of Boston, who acted for a time as one of the agents for raising funds in behalf of the N. E. Female Medical School, is out in a pamphlet which purports to be an exposition of the dishonesty and frauds of some of the Directors of the college. Mrs. Gassett charges them with being thieves and humbugs.

Gambling to the Teeth.—A Texas paper relates the following incident as having recently occurred in the "Lone Star:" "An individual, a few nights ago, being put to for a stake, took from his mouth a finely polished set of teeth on a gold plate, and pawned it for a few dollars to continue the game with." We have heard of "fighting to the teeth," but never before heard of gambling to the teeth.

Heavy Fees.—Dr. Dimsdale, of Hertford, visited Russia to inoculate the Empress Catherine and her son, for which service he received the rank of Baron of the Empire, &c., besides a pension of £500 per annum, and a present of £12,000.—Dupuytren made his last professional visit to one of the Rothschilds, who gave him 100,000 francs for attending to his fractured femur.—Sir Astley Cooper, after performing the operation of stone upon Mr. Hyatt, a West Indian merchant, was presented with a fee of 1000 guineas.

The Income of Sir Astley Cooper in Early Life.—"My receipts," says he, "for the first year were £5 5s.; the second £26; the third £64; the fourth £95; the fifth £100; the sixth £200; the seventh £400; the eighth £600; the ninth £1100."

Dr. Pond, of Rutland, N., has invented a pill-making machine, which manufactures any quantity of those articles in a day.

Of twelve American surgeons who have gone out to join the belligerent armies in Europe, nine have attached themselves to the service of the Czar, and three have attached themselves to the Allies.

London papers announce the death of Dr. Archibald Arnott, in the eighty-fourth year of his age. He was Napoleon's last medical attendant. Napoleon expired with his right hand in that of Dr. Arnott.

The Gazette Medicale complains of the difficulty experienced in obtaining a sufficient number of military surgeons for the armies in the East.

During the months of January and February, according to the Inspector General's Report, there were issued by the Purveyor to the Forces in the Crimea, 560 doz. port wine; 131 doz. sherry wine; 630 bottles rum; 8 gall. rum; 688 bottles brandy; 1724lb tea; 7920lb sugar; 2111lb preserved meat, &c., &c.,

Wong Tun is the only native of China who has ever received the honor of medical graduation in Edinburgh.

Turkish Order of Knighthood,—Dr. C. T. Jackson received by the last steamer from Europe the patent and decoration of the Order of Knighthood conferred upon him by the Sultan of Turkey, Abdul Medjid, for services rendered by his discovery of Anæsthesia to the armies of the Allies. This is one of the fifteen decorations of this order lately conferred by the Sultan on those who have rendered service to the empire.

Change.—Prof. Torrey has resigned the chair of Chemistry in the College of Physicians and Surgeons of New York, and Dr. Leconte, of Georgia, has been appointed in his place.

Dr. P. Claiborne Gooch, of Richmond, Va., the founder of the Stethescope, recently fell a victim to Yellow Fever in Norfolk, where he had gone on the humane mission of affording succor to the distressed population of that city.

Statistics of Paris.—The population of Paris may be considered, with the environs, as 1,200,000. The average daily consumption of bread in the city is a million of pounds, or a pound for each person; but, as every workman eats three pounds a day,

it is found that the consumption by women, by children, and by the aged, which is considerably under a pound a-piece, furnishes an ample compensation. The climate is calculated to require a person in good health to consume one pound of meat, one and a half of vegetables, and one and a half of bread, with a bottle of claret, or two bottles of beer. The consumption of bread diminishes in years of abundant wine yields, and *vice versa*. A heavy rise in the price of bread increases the number of deaths very perceptibly. The 40,000 cats and 70,000 dogs of the city of Paris, eat six millions pounds of bread a year. Unwise economists have proposed their destruction, in view of the saving that might be effected; but it is clear that it would only provide for six days' consumption out of three hundred and sixty-five. There are 601 bakers in Paris, who are divided into four classes:—the first, including those who use more than four bags of flour a day; the second, third and fourth, those who use three, two, and less than two a day. A bag contains 314 pounds of flour, and furnishes 408 pounds of bread. Any baker who puts more water into a bag full than is necessary to raise it up to this standard weight; or any one who adulterates his flour with mixtures of carbonate of magnesia, bi-carbonate of soda, or powdered alabaster, is punished by a fine of thirty francs and a week's imprisonment. Frauds in bread, however, are extremely rare. There are 500 butchers in the city. The number of persons in Paris who abstain entirely from meat, is so large, that the average consumption of the city is reduced to three ounces a day for each person. It is the class that goes without meat that fills the hospitals. The average wine yield of France is 1,000,000,000 gallons—two-thirds of which is consumed in the country, and one-third exported. Of the 333,000,000 gallons annually drank in France, Paris claims 40,000,000—that is five times as much as its proportion of population entitles it to. Of this quantity, 7,000,000, or one-sixth, are supposed to be added, either in the form of water, or of decoctions purely artificial. The government does what it can to punish and prevent frauds of this sort, and it keeps in its employ sworn and patented tasters, whose palates possess a sort of humming-bird delicacy.

Record of Medical Science.

Clinical Lectures on Fever. Delivered in the Meath Hospital, Dublin. By William Stokes, M. D., Regius Professor of Physic in the University of Dublin.

Lecture XIII.—1. Congestion, with more or less consolidation, in cases of what is called "diffuse inflammation," "erysipelatous inflammation," or, by some, phlebitis.

2. Similar or nearly similar conditions (so far, at least, as we are taught by pathological anatomy) in cases of purulent absorption, with or without manifest phlebitis.

3. The intercurrent disease of the lung in cases of the eruptive fevers, when they are of the low, putrid or malignant type.

4. Congestions and semi-consolidations of the lung, as intercurrent affections in typhus fever.

5. Analogous conditions arising in the course of the non-maculated or the so-called "typhoid fevers."

6. The disease occurring in connection with delirium tremens from excess. In such cases, you will often find a group of asthenic local diseases, which are generally seated in the stomach, heart, the bronchial membrane, the parenchyma of the lung, and even the pleura. In some instances we find that the patient has also typhus fever.

7. Rapid, extensive, and complete consoldidation of the lung occurring in the course of a typhus fever. In some instances the patient dies asphyxiated; while, in others, a portion of the pulmonary structures falls into sphacelus, and death takes place with the symptoms of acute gangrene of the lung.

Such are some of the more prominent cases which have been classed under the head of typhoid pneumonia. There is another of which we have had many examples, and yet I do not wish you to take what I am going to say about it in any other way than in the light of suggestions. The case, as I said before, is by no means uncommon. The patient is attacked with the usual symptoms of typhus fever, and he comes into Hospital after two or three days' illness. There is nothing about him to make one think that his disease will not run the usual course of the epidemic of the day, and we are prepared to expect a fever of at

least a fortnight's duration. On admission he may have no symptom which would call attention to his chest; but, as early in some cases as the beginning of the fourth, and in others of the fifth day, it is discovered that the upper lobe of one lung is solid, or nearly so. The clavicle is quite dull on percussion, so is the scapular spine, and the dullness extends to the line of the mamma. This discovery has been so often made accidentally, that I am sure many of such cases have passed unnoticed, at least, where the attendant is not well informed as to the insidious nature of typhous local diseases, and does not make it a practice and a duty to examine daily, as far as he can, the condition of every organ.

But the most remarkable circumstance in these cases is, that the constitutional disease seems to be cut short. The expression of fever leaves the countenance; the peculiar color or hue of typhus disappears; the eye becomes bright and intelligent; the tongue cleans; and the pulse comes down to a natural state. And thus we have seen patients so altered in the course of twenty-four hours, that one had some difficulty in recognizing them. All the symptoms of typhus were gone, and nothing remained but the consolidation of the lung. And this, too, is not attended with any notable suffering. There may be a little cough, some dull pain, or an inability to lie on one side; but that is all. The respiration is scarcely, if at all, accelerated; in fact, it would seem that there was no irritation or excitement of the organ; and the case is another proof of how much less the sufferings in disease are connected with the mechanical than with the vital conditions of organs.

This local disease, too, is generally easily managed. A small blister or two, the application of the tincture of iodine, and the exhibition of a little hydriodate of potass, with or without a tonic, will remove it. Indeed, the cure is often so rapid, that I have thought that our remedies had little to do with the result. How are we to look at such cases? That they are not examples of inflammation of the lung is plain; and it appears probable that, if this local disease had not occurred, the patient would have gone through the course of the fever of the day. Does it not seem as if the constitutional disease exhausted itself, as it were, in the production of the local affection, just as, in certain cases of simple variola, we see the fever to subside on the appearance of the pustule? I do not know whether such cases have been observed elsewhere, but of their existence we have had abundant proofs. It is worthy of remark, too, that when we compare these cases with the ordinary forms of typhus, attended with secondary disease of the lung, the local affection is here developed at an unusually early period; and it may be, that, in the more protracted cases of fevers, the nature of which is to develope local affections, the periods of this development and of the cessation of the fever may also be

coincident. We do not, however, find that this is so common as to establish a rule. Let us, assuming that these curious cases were really examples of typhus with a secondary deposit, again compare tuem with the more ordinary forms of the disease, and we shall find that they want two important characteristics of the longer fevers; one, the successive production or the simultaneous production of various local diseases; and the other, the occurrence of that secondary inflammation or irritation of the parts in which the deposit takes place. That the latter circumstance is one of great weight in relation to the preventing or delaying of crisis, it is impossible to doubt. As to the case of successive or simultaneous production of local diseases, this, at all events, marks a more severe and complicated disease.

Gentlemen, I will not here enter into the wide subject of crisis in fever; yet, I may point out to you, as a matter well worthy of investigation, the possibility of the occurrence of crisis by other modes than those which are generally enumerated; thus we may have a crisis without sweating, or diuresis, or hemorrhage, or diarrhœa, but which takes place by an internal and silent change in the condition of an organ, and yet a change which will, or may, itself spontaneously disappear.

Here let me warn you against the error which so many fall into with respect to these various cases of disease of the lung arising in the course of some form of constitutional disease or fever. They are usually set down as pneumonia, typhoid pneumonia by some. Now, the name itself would be of little moment if its adoption did not lead to errors in practice. And, although it cannot be affirmed with certainty that in none of these cases is there pneumonia, yet we have good grounds for believing that, in many of them, inflammation, as the term is commonly understood, is either absent from the first, or, if it occurs, that it is only secondary to a special lesion induced by some form of constitutional disease.

It is extremely difficult to present to you any well-defined classification of the various forms of diseases which have been described under the head of typhoid pneumonia, or to draw the line between simple asthenic inflammation of the lungs and those conditions which have been described from an early period under the terms of bilious, putrid, or typhoid pneumonia. And observe that when I make use of the term asthenic pneumonia, we refer more to the condition of the general system than to the activity or inactivity of the local disease. For, so far as local inflammatory action is concerned, there is abundant proof that it may originate and proceed with rapidity, and even with vehemence in the very last periods of life, so that the disease may be sthenic *quoad* the local condition, and yet the case itself be asthenic in reference to the general state of the economy. Much of the confusion with

regard to this subject has arisen from the circumstance, that too great weight was attached to the presence of certain physical signs, which were taken as always indicating similar conditions. The succession of the signs of crepitus, dullness, cessation of vesicular breathing, and its replacement by bronchial respiration, has been admitted to indicate a simple pneumonia, in which the local disease is the principal pathological condition, and the fever only a secondary one. But it is certain that this train of phenomena, or some modification of them, may occur under exactly opposite circumstances, the local disease being symptomatic of the fever, and not the fever of the local disease. And there is the strongest reason for believing, that even though the mere anatomical condition of the lung in the two cases be similar, yet there is an essential, a vital difference, and that practically we cannot deal with the local disease in the latter case as if it were an original affection. This applies to all those cases of disease with the physical signs of pneumonia, which are secondary to any forms of fever, whether it be typhus or typhoid, whether it be variola or erysipelas, purulent poisoning of the blood, glanders, malignant scarlatina, or malignant measles. In these cases, even though the physical signs accurately correspond with those of the typical pneumonia, which, by the way, is by no means always the case, we must believe that we are dealing with a special condition of parts, and a condition not only special as compared with ordinary pneumonia, but having also another speciality, namely, that which is derived from the parent malady.

In the present state of our knowledge, gentlemen, we cannot declare that any special anatomico-pathological condition exists by which we can distinguish these secondary diseases one from the other; but we may say this much, that practically they all appear to agree in being indicative of an asthenic state of the system, and that, therefore, the supervention of their physical signs at any period of those various diseases, must not be permitted to divert your attention from the general condition of the patient, or make you proceed to treat a case as one of sthenic pneumonia because it has some, or even all, the physical signs of that condition. Do not suppose that I am taking up your time unnecessarily by insisting on these points, for they lead us directly to deal with one of the greatest, if not the greatest and most wide-spread error in the practice of medicine, namely, the treatment of all local, acute, and febrile diseases as inflammations. Here is a group of acute, local, and febrile diseases, and not only this, but a set of cases exhibiting some or all of the physical phenomena of acute pneumonia; and yet, if we subjected them to the ordinary routine treatment of inflammation, the worst consequences would almost certainly follow. You must learn to look at the antecedents and the accompanying general phenomena

of these diseases, and set your face against the adoption of any treatment which is based on the doctrine that they are original inflammations. I believe that the erroneous views to which I allude, are, I am happy to say, every day becoming less and less frequent, (thanks to our improved system of clinical instruction,) and the independent spirit of investigation which now animates so many of our students. Notwithstanding, they are still too often acted upon, and over and over again patients who have enough to contend with as the victims of some fell fever or other constitutional disease are lost, or assisted to their death, by the adoption of a local or general anti-phlogistic treatment, in consequence of the physical signs of a pneumonia being discovered; their stimulants are withheld or withdrawn; tartar emetic, or mercury, or even blood-letting is rashly resorted to; and it often happens that, even though the physical signs of the pneumonia are removed or modified, the patient sinks from the combined effect of the original disease and the exhaustion produced by this ignorant and benighted treatment. I do not think that any of you will fall into these or similar errors after what I have so often said; it will, at least, not be my fault if you do.

Let us now consider what we may term the parenchymatous affections of the lung in fever, or, if you will, the typhus disease of the pulmonary structure. It may be stated, generally, that whatever be the differences in the various cases of this affection in typhus, the local disease follows the general law of other lesions secondary to the fever; that is to say, it agrees with them in its mode of invasion, its latency in the earlier periods of its development, its frequent spontaneous retrocession, and, lastly, in its pathological effects. It is quite true, that, as compared with the best marked examples of acute sthenic pneumonia, it is not wanting in any of the physical signs of that disease taken singly, but it is generally different from it in the order or arrangement, as it were, of these physical signs. And, indeed, I think it is an extremely rare circumstance to observe in the course of a typhus fever the rise, progress, and retrocession of a pneumonia which has passed into hepatization, as we so continually see in ordinary cases of the disease. I have already drawn your attention to those curious cases of consolidation of the upper lobe of the lung; now, whether these be genuine examples of an arrested typhus or not it is difficult to say, but their whole history and progress is very different from that of ordinary pneumonia; and I repeat, that there is nothing more rare than to see in the course of a typhus fever the regular succession of phenomena with which Laennec has made us so familiar, as indicating the several successive stages of an acute pneumonia occurring under what may be called its normal conditions.

The most common case is the occurrence, generally, at an early

period, especially in the maculated forms, and often at a later period in the non-maculated or so-called typhoid fevers, of a well-marked crepitating râle in the lower lobes of one or both lungs; it is generally much more extensive and distinctly marked in one lung than in the other. The amount of dullness is seldom very great, and we find the disease, as it were, to linger, and for days together to show no disposition either to produce solidity of the lung on the one hand, or to proceed to resolution on the other. The disease is often quite latent, and is only recognisable by careful physical examination; and its discovery, as you will readily understand from what I have said before, is sometimes an unfortunate circumstance for the patient. In the present state of our knowledge we must believe this condition of the organ to be either the result of a certain amount of typhous deposit into the lung, or of a special state, an inflammation, if you will, which is, however, under the general law of the fever, partaking of its specific character, and capable of spontaneous retrocession. It is seldom attended by pain or hæmoptysis, and it constantly exists without any important modification of the general symptoms of the case.

The second form of the disease is of a more serious character, and seems to be connected either with an original pyogenic disposition, itself secondary to typhus, or we may suppose that the typhoid deposit undergoes a rapid purulent transformation, so that in this way a condition of the lung is rapidly established, having some resemblance to the third stage of pneumonia, as described by Laennec. I have not myself seen a sufficient number of these cases to justify me in speaking very decidedly as to their physical signs, but I think I have seen enough of them to warrant me in believing that the course of the disease is different from that of ordinary suppurative pneumonia. We have not observed the intermediate stage of well-marked hepatization between that which is characterized by the occurrence of early râle on the one hand, and the signs of interstitial suppuration on the other. The complete dullness and the bronchial respiration which accompany the third stage of pneumonia, as described by Laennec; we have not observed, the physical signs being principally a persisting râle passing from a fine into a large crepitus, and semi-dullness on percussion. On dissection, the lung is found soft, friable, of a greyish-red color, but still very permeable to air, though infiltrated with purulent matter. It is as if the purulent secretion took place coincidently with or immediately after the first or congestive stage. Some of the patients have had sweatings and a sanguinolent and somewhat sanious expectoration; but we have not hitherto observed the ordinary prune-juice sputa in these cases. I have seen this disease in connection with purulent deposits in the neck, and posterior mediastinum, but it may occur without the

formation of purulent matter in any situation other than the lung; it may supervene in the advanced periods of the case, and at a time when the patient seems about to recover; or it may come on much earlier, and when the skin is thickly covered with the petechial eruption. The last case is the most formidable; though one attended with the greatest danger, the disease is, however, not always fatal, and we have had several cases in which recovery took place. I need not say that they were all treated upon a tonic and stimulating plan, in addition to which we employed dry cupping and blisters.

The last case of which I shall speak to-day is by far the most formidable of the pulmonary affections of typhus; it is characterized by the sudden, complete, and singularly extensive consolidation of the lung. In the course of twenty-four, or even, sometimes, of twelve hours, the most extensive and complete dullness may be produced in a lung which had been previously free from physical signs, or, at most, had only exhibited some of the ordinary bronchial râles of typhus. We have thus the signs of complete hepatization, unpreceded by the crepitating râle; the disease begins by consolidation, and then one of two results follow: either the patient dies speedily, generally with loose râles in the opposite lung, combined with tracheal effusion; or, after a day or two, he begins to expectorate a horribly fetid matter, and we discover by the stethoscope that a large cavity has formed in the lung. This is a true gangrenous cavity formed in the very center of the solidified mass, and the disease has a close pathological analogy to the process of acute mortification which has been described as occurring in some of the worst cases of the typhous disease of the intestinal glands.

Let us now pass in review the circumstances in which these forms of disease occur; for, when we compare them with the ordinary conditions of acute primary pneumonia, we cannot but admit that they indicate a lesion of a very different nature.

In the first place, the physical signs are preceded by fever; and it may not be till many days have elapsed that the symptoms of lung affection, as it were, spontaneously arise. Secondly, the fever is obviously an essential fever; it may occur with or without petechiæ, and other complications may or may not be present. Thirdly, the disease sets in without any apparent external exciting cause. Fourthly, when the purulent form is observed, it appears to be, as it were, the second stage of the affection; and I may here remark, that on dissection, we rarely, if ever, find what is termed concrete or non-concrete purulent matter. Lastly, the invasion of one form of the affection may be sudden, and the signs of extensive and complete consolidation be among its earliest phenomena. It is in this case, too, that if time be allowed, large eschars forming cavities, which may commmunicate with the bronchial tubes, are liable to occur.

I may remark here, that in two cases of this rapid consolidation, the gangrenous eschar did not communicate with the bronchial tubes. One was a case of typhus, in a man who had long before suffered from gangrene of the opposite lung; the other occurred in a case of what is termed the erysipelatous or diffuse inflammation.

Lecture XIV.—I endeavored to convey to you, at our last lecture, that the conditions which have been described under the head of typhoid pneumonia were probably examples not only of a pathological but of an anatomical state of parts different from that which is found in the simple original inflammation of the lung. And it is a great deal easier to say what they are not than what they are, to state their negative rather than their positive characters.

Now I wish to state to you here that a certain change has occurred in our opinions as to the origin of the so-called typhoid inflammation of the lung. We at one time held that it was the coexistence of gastritis or enteritis which gave to the pneumonia the typhoid character. This view was held by us before we had, by that imperceptible power of conviction which arises from experience, admitted the two following principles in their entirety:

1. That symptoms which are diagnostic of local disease, where the patient has not an essential fever, are either altogether valueless or much lessened in value when such a condition exists; and

2. That the gastric or gastro-enteric lesion is rare even as a secondary disease in fever; so that when irritation of the structures of the intestinal tubes occurs it is a remote, tertiary, and accidental phenomenon.

Our present opinion on this matter is in general the following: That in cases in which there are, in connection with the signs of typhoid lesion of the lung, evidences of gastro-intestinal disease, both the pulmonary and abdominal lesions spring from the one parent condition, and that so far from the specialities of the pulmonary being derived from the accidental complication with the abdominal disease, both have a common character originating in the same source, I am quite sure that a large proportion of those cases described as asthenic pneumonia depending on gastro-intestinal complication, have been examples of essential fever, with the two affections coexisting as secondary lesions.

We have seen that in these cases, I will not say of typhoid pneumonia, but of typhous or typhoid affections of the lung, the various physical signs of pneumonia, singly considered, may be present, and are actually often to be found. They fail, however, very frequently to present themselves in the regular order or succession which is observed in true acute pneumonia.

Now let us inquire whether there is any physical sign peculiar to these cases of typhous pulmonary affections, which does not occur, at least as the rule, in ordinary pneumonia; and I do not know of the existence of any such, unless it be the sign of tympanitic resonance over the diseased lung, a condition first noticed by Dr. Hudson, of this city, and to which he attaches some importance. Dr. Hudson states that in certain cases of typhoid consolidation of the lung, the sound on percussion was very different from that observable in the ordinary condition of hepatization. He describes it as "a tympanitic clearness over the solidified lung without air being present in the pleura;" indeed he goes so far as to say that in one case the tympanitic clearness on percussion existed fully to the same degree and of the same kind as in pneumo thorax; here the lung was found perfectly solid throughout, with the exception of a small extent of the anterior and postero inferior parts which was still crepitating.

It is very difficult to understand what condition of parts could have caused this singular tympanitic clearness over a solidified lung. When we speak of tympanitic resonance, it must be always borne in mind that the tympanitic sound does not always imply clearness on percussion. When you have a cavity in the center of a solidified lung, or when you have hepatization of the left lung, in connection with flatulent distention of the stomach, the sound, on percussion, though dull as compared with that of the healthy lung, has a distinctly tympanitic character; to this we have long been in the habit of giving the name of tympanitic dullness. I have never found it, however, to simulate the tympanitic resonance which occurs in pneumo-thorax, or in dilatation of the air cells; it is inferior in degree, and different in character. Dr. Hudson met with four cases, in which the observation of this phenomenon was followed by dissection. One was that of a man who died of extensive inflammation of the left lung in the Meath Hospital, in the spring of 1832, in which, at the close of the case, from the hollow sound on percussion at the lower part of the left side, it had been previously quite dull, a pretty general opinion existed that a pneumonic abscess had formed and burst into the pleura. On dissection, the side having been punctured, no air escaped; the lung was red and solid, but without abscess, and the pleura was adherent over two-thirds of its extent. I must, in justice to myself, state that in this case I never entertained the idea that an abscess had opened into the pleura. In another case the lung was found hard and solid from chronic pneumonia; and in the two remaining cases the condition of parts was similar to that which was presented by that first detailed.

Dr. Hudson admits that in three of those cases the tympanitic sound might be explained by reference to the distended state of the stomach; but in the fourth he thinks that the explanation

might be found in the facility with which the vibrations of the air in the bronchus, and its larger divisions, may be supposed to be communicated through a lung in that condition, that is to say, solid throughout; and, therefore, not permitting the loss in a mixed medium of solid and healthy lung of such vibrations.

I am quite prepared to admit that with extensive solidification of the lung, the dull sound, on percussion, may yet have a tympanitic character; but I have seen no case in which this sound could be confounded with that of pneumo-thorax, or of dilatation of the air cells. With reference to the bearings of this question upon the signs of typhoid pneumonia, I can only at this moment remember two cases which are worth detailing to you. In one tympanitic dullness did occur over the diseased portion of the lung, without our being able to account for it, by any accumulation of air, either in the pleura or in the stomach. The case was of a low, putrid character, and I remember suggesting it as a matter just possible that there might have been a typhoid pneumatosis developed in the diseased lung; but I am sure that we were not able to establish the existence of such a condition on dissection: the case occurred a good many years ago.

In the second case, which was one of manifest typhus fever, the posterior portion of the right lung became solid or nearly so, while the anterior face of the organ preserved its vesicular respiration. Now we found that over this portion of the chest, that is, over the front of the thorax on the right side, the sound, as compared with that over the opposite lung, was morbidly clear; it was true tympanitic clearness, not dullness, and it continued for three or four days, and gradually disappeared with the resolution of the posterior solidity: this case occurred in the Hospital last year, and was seen by Dr. Hudson himself. I confess I am quite at a loss to explain the nature or mode of production of this phenomenon. My friend Dr. Lyons mentions to me that in a case of asthenic pneumonia, occurring in a patient of intemperate habits, which we saw in consultation, the anterior superior part of the left lung presented for a couple of days a condition of morbid clearness, but subsequently became engaged in the general consolidation of the organ.

Dr. Lyons is disposed to regard the abnormal clearness which occurs in these cases as the result of the increased pressure of the respiratory column of air in the still permeable portions of the pulmonary cells, which he considers in certain cases become from this cause expanded beyond their natural volume. His views are that the inspired air presses with a certain force on the whole pulmonary surface, and that if a portion of this service becomes impermeable to air from solid deposite, occlusion of the tubes which forced it, or other cause, the remaining portion of the pulmonary tissue is acted on by the whole of the inspiratory force,

before which it is thus made to expand; this portion of the lung may thus be considered to be in a condition of temporary emphysema, and so gives a correspondingly clear sound on percussion.

There is a circumstance in connection with the resolution of these typhoid or typhous diseases of the lung, different from what is commonly observed in sthenic pneumonia. You know that the true inflammatory hepatization rarely disappears in a sudden manner. It subsides gradually, and the transition-state between dullness and clearness on percussion is generally marked by the 'crepitus redux.' In the cases before us, however, and especially where the disease is secondary to typhus fever, the resolution, as I have before stated to you, is often singularly rapid, and is often unattended by the crepitus of resolution. If, then, you consider the state of solidification simply, we find it on the one hand to form without the crepitus of the first stage of pneumonia, and on the other to disappear rapidly, and without the rule of resolution. Thus we are permitted, as it were, to witness the silent and spontaneous development and retrocession of one of the secondary diseases of typhus.

This change from the state of consolidation to that of permeability to air, this rapid change, unattended by the crepitus and resolution, probably shows that the real disease was one unconnected with inflammation, either as a primary or a reactive condition.

You will remember that I suggested to you that some of the cases which have been described as typhoid pneumonia, might be held as examples of an aborted typhus. These were characterized by early consolidation, early disappearance of the typhous state, and often spontaneous subsidence of the local disease. I cannot help thinking that between such cases, and those in which the general disease runs its usual course, there is another class or category of cases in which the progress of the merely pulmonary disease is marked, more or less, by signs of irritation or inflammation of the lung, which inflammation or irritation is either reactive or specific, or both reactive and specific. And I apprehend that these cases which, as it were, float between the aborted and the perfect typhus, are much more numerous than might be supposed; and in such instances the case is often treated throughout, without a suspicion of its being really an example of typhous disease having been entertained.

What has been now said should impress on your minds that rule in practice which I have so often urged upon you, namely, that the rules of diagnosis of local inflammatory disease which are good in ordinary cases, lose their value in a great measure when the patient has typhus fever. This was long ago proved by the researches of Louis on the condition of the brain in fever, and it was the non-recognition of this fact which constituted one of the

greatest errors in the system of Broussais. I have told you that if you gained nothing during the session but the knowledge and full appreciation of this great principle, your time would have been well spent. How many cases have we not had of headache, delirium, watchfulness, or its opposite, coma—yet without encephalitis; and so it is with the remaining cavities—symptoms of functional alteration are met with in connection with the cerebral, pulmonary, circulating, and digestive systems in fever. They may or may not be attended by organic change, and that organic change, when it does exist, is not necessarily inflammation; and we cannot, I believe, lay down any satisfactory rule of diagnosis which would show that in one case of local functional disturbance there was organic change, and in another that there was not. But this much we do know, that those groupes of symptoms which are diagnostic of local inflammation in a case which is not fever, cease to be so when they occur in a case of typhus. Let this principle be ever present to your minds, for it is impossible to exaggerate its value. Long ago it was acted on empirically by the best physicians, who refused to adopt antiphlogistic measures in treating the local symptoms in typhus, and who employed stimulants irrespective of them, when the general condition seemed to demand such treatment. It now comes before you as the result of an extended and accurate pathological investigation, and the study of the pulmonary phenomena, as we have seen, enables us to go a step further, and to declare that not only are the symptoms of local irritation doubtful or illusive, but that even the physical signs of a pneumonia, when occurring in a case of typhus, are not to be taken as proof that a local inflammation has occurred.

If these things be true so far as our typhus is concerned, it would appear probable that in other acute diseases, under the influence of a law of periodicity, and, perhaps, in many that arise from the operation of an introduced poison, the same circumstances may be found, so that we might apply to a much larger circle of diseases those principles as to the secondary local affections, which appear applicable to typhus fever.—*London Lancet.*

Diseases of the Eye.—The treatment of strumous ophthalmia naturally divides itself into constitutional and local, which may be dwelt upon separately, for the sake of arrangement, though at the same time it must be born in mind that they bear an intimate relation to each other; for, while it must be admitted that all these cases are in some measure due to constitutional causes the local disease often persists long after the original and exciting systemic cause has passed away. There are three principal di-

gressions from the normal standard that I have particularly noticed as favorable for the incubation of this form of ophthalmia. The first of these I would designate the 'inflammatory type.' It is rapidly developed, the local symptoms are acute, there is severe pain in the eyes, attended with profuse lachrymation of a scalding character, red, swollen lids, and spasmodic closure. The constitutional symptoms exhibit a hot and dry skin, a rapid pulse, foul tongue, loss of appetite, arrest of all the natural secretions, constant restlessness. The treatment, under these circumstances, consists chiefly of salines, antimonials, mercurial purgatives, and the use of the warm bath: this latter is peculiarly useful in such cases. The diet must be light, and not either stimulating or too nutritious. At the same time care is required not to persevere in this plan too long, not in fact a day longer than the state of the secretions indicate its propriety, otherwise a feeble condition is induced, and the disease continues in a subacute or chronic state. Much discrimination is needed in making out these shades of difference, and success in the treatment mainly depends upon our doing so.

The second class of cases that seem to deserve special notice may be denominated the 'irritable type.' In these cases the mucous membranes are congested and disordered, and their secretions increased in quantity and vitiated in quality; the tongue and lips are aphthous and swollen; the nose excoriated; the skin inflamed: combined with this there is often a tendency to prolapse of the rectum and chronic diarrhœa; this condition frequently follows one or other of the exanthemata, and may be kept up by the presence of worms. You may readily recognize this form of disease by the general outpouring from the mucous outlets. Unsuitable food and want of cleanliness are fertile sources of this deranged state of the mucous membranes.

In the treatment of these cases all causes of irritation must be removed; if worms are suspected, one or two drastic aperients may be required. The secretions may be improved by a mild mercurial and an alkali. The diet must be carefully attended to; many of these symptoms result from crude, ill-digested food, impropor in quality or superabundant in quantity. The children of the poor are very much mismanaged in this respect. At the Ophthalmic Hospital, I always gave a laconic, but, as I believe, an effective formula—"stale bread-and-butter, milk-and-water and a little meat once a day." It often happens, however, that after all these means have been carefully employed, the irritation of the mucous membrane remains, which purgatives only seem to increase. Under these circumstances I found the utmost possible benefit from Battley's solution, regulating the dose by the age of the patient, allowing a minim for each year, and giving it once or twice in the twenty-four hours. It seems, in these cases,

to act like a charm, removing the diarrhœa, checking the other morbid secretions, improving the appetite, allaying restlessness, and subduing the most urgent and distressing symptoms, namely, the extreme photophobia. Much prejudice exist in the minds of many against the use of opium in any form in children; but extensive experience has convinced me of the great value of this remedy in suitable cases. Warm baths soothe and allay irritation. Tonics of any kind are not well borne, and seem to aggravate all the symptoms.

The third division of these cases may be termed the 'asthenic type.' It is found most commonly among the children of our poorer population. The chief symptoms are emaciation and pallor, with a rapid and weak pulse, cold extremities, moist tongue, frequent perspirations, and are quite compatible with, and coexist with a considerable amount of ophthalmia and extreme photophobia. It is in this group of cases that we commonly find the intermittent form of the disease. Towards evening the child will open its eyes, and play about as usual; but the symptoms will recur with the same intensity the next morning. I have observed that a tonic treatment is most valuable in these cases; they often come before our notice at the Ophthalmic Hospital. We find such patients suffering from the effect of bad ventilation and insufficient and unwholesome food; and sometimes, in addition to this, active antiphlogistic treatment may have been employed, with a view of subduing local inflammation. It is remarkable how rapid a change is produced by the adoption of every available means for developing power in the system. Where intermittance of symptoms and periodicity can be traced, quinine seems to act like a specific; the dose must be, of course, regulated by the age of the patient, but rather a large dose can be borne, and seems to be required. Where the fibre is very lax, and the limbs feel flabby, steel is useful, and seems to be indicated, and the two may sometimes be combined with great advantage. The combined salt of iron and quinine is a convenient form for its exhibition. At the same time a liberal diet, with a moderate amount of stimulus, must be given; and fresh air, and, if possible, sea air and all such means as are calculated to develope power.

Many medical men, judging by the severity of the local symptoms, and losing sight of the general debility of the system, treat such cases antiphlogistically, limiting the patient to spoon-diet, and acting upon the various secretions. When a case so treated presents itself suffering from all the adverse circumstances which a feeble state of system has induced, it is often remarkable to observe the rapid improvement that takes place under an entire change of management. Tonics and a liberal diet may be given altogether regardless of the severity of the local symptoms and of the intensity of the intolerance of light. If this plan can be fully

carried out, its favorable effect is speedily observed; but our chief difficulty consists in counteracting the enfeebling influences by which such cases are surrounded, such as impure air, unwholesome and insufficient food, etc. When the strumous diathesis is very marked, the glands enlarged, upper lip thickened, cod-liver oil may be given with advantage, either alone or combined with steel. If the tongue is foul or the secretions disordered, it is a good plan to commence the treatment with an emetic, and then to exhibit tonics. Where the secretions are very abundant, opiates may be combined with tonics very advantageously. Where this asthenic condition is complicated with much mucus and cutaneous irritation, such as I have before described, with swollen nose and lips, enlargement of the glands, acrid secretions, and a cold, pasty, flabby surface, I have observed remarkable improvement in all these symptoms, from the exhibition of arsenic, in doses proportioned to the age of the patients. About two minims of the liquor arsenicalis is the dose I generally give, three times a day, immediately after meals. The effect must be carefully watched, and the medicine must not be given at any one time for a longer period than a fortnight or three weeks. The beneficial effect is often rapid and striking, and it may be given in a state of system for which no other medicine seems to be quite suited. As regards the use of mercury in these cases, I believe that in by far the greater majority it is very injurious; but instances do occur, though they are rare and difficult to detect, in which this medicine is useful. I can only indicate them as belonging to the class of strumo syphilitic, in which some syphilitic taint is engrafted upon and combined with struma and produces a train of symptoms in many respects resembling some forms of tertiary syphilis. I have met with cases so closely resembling the more remote effects of a primary sore, that if it were not for the age of the patient, you might suppose it was the result of the direct influence of this poison. I am not now speaking of those cases that occur soon after birth, and are manifestly syphilitic, but rather of such as appear at ages ranging from about seven to twelve; the cornea and iris and even the deeper textures are often involved, and the intolerence of light is very extreme. Minute doses of mercury, as for instance one or two grains of the gray powder, or one ninetieth of a grain of the bichloride, twice or three times a day, seem to act very beneficially both upon the general health and the local symptoms. I must, however, again urge the extreme importance of correctly diagnosing this peculiar complication, otherwise more harm than good will be done by the exhibition of this medicine.

Before I leave the subject of constitutional treatment, I must not omit to mention that most efficient of all means for restoring strength in a strumous diathesis—I mean change of air, particu-

larly to the sea—this expedient will sometimes effect a cure when all else has failed. Not only is it useful in removing present mischief, but it often prevents a relapse, a strong tendency to which constitutes one of the most distressing features of such cases.

We may now pass on to the consideration of the local treatment of strumous ophthalmia, a point second only in importance to the constitutional, since we find a judicious combination of topical means with general treatment essential to success.

Local treatment may be arranged conveniently under the following heads: Local depletion; Counter-irritation; Local stimulation to the lids and to the eye; Local sedatives. In the earlier stages of an attack, where the symptoms are acute, the vessels full and numerous, and of bright-red color, or in a sudden relapse of an old standing case, in which you can trace fresh organized deposite or a false membrane on the cornea, the abstraction of blood, by means of leeches, is often useful; they may be repeated, with advantage, once or twice, but I have usually found it useless to continue this plan for any time; all the benefit likely to accrue from it is very speedily obtained; a prolongation of local depletion is apt to enfeeble the system, without any corresponding subsidence of the local disease. In less acute cases, where the vascularity is not so prominent a symptom, or where it has been diminished by local depletion, blisters are very useful; they must be frequently repeated, and, in mild cases, will effect complete relief of the more prominent symptoms. Mr. Tyrrell dwells strongly upon the advantage of this form of temporary counter-irritation in relieving intolerance of light, and seems to rely upon it almost exclusively as a local means in these cases. My own experience does not allow me fully to subscribe to this view. In the more severe cases, blisters fail; and in some forms of the disease, in which mucous and cutaneous irritation coexist, they seem to be decidedly contra-indicated. A modification of this plan, though possibly acting in a very different way, has recently been strongly advocated. It consists in applying some powerful local stimulant to the eyelids. Some use for this purpose the solid nitrate of silver, moistened, and brushed lightly over the lids; others paint the tincture of iodine on the lids. Whichever is selected, care must be taken that it does not find its way into the eye; and it usually requires to be repeated every four-and-twenty hours, for a few days, till a decided impression is made on the skin. The advocates of this plan attribute its efficacy to the direct influence of the stimulus upon the extreme branches of the fifth pair of nerves: upon an abnormal irritability of which they believe the photophobia to depend. Whether the explanation be correct or not, I have no doubt of the advantage of the plan in some cases. I have tried it very extensively, and have observed that where it is useful its effect is very rapid and complete, but that it is only in

a certain limited number of cases; in many others, it has had no appreciable effect. The important and difficult point to ascertain is, some guide or indication to its applicability—whether the use of this method must be tentative and empirical, or whether we can select cases in which a favorable result may be predicated. It appears to me that this method is peculiarly applicable to those cases in which vascularity and other evidence of diseased action are slight, and photophobia exists as the prominent and principal symptom, and in which the disease is not of very long standing. I generally use the tincture of iodine in preference to the nitrate of silver, because it occasions less pain, is less liable to produce vesication, and is equally efficacious. With regard to the employment of solutions of the nitrate of silver, and other local stimuli, to the conjunctival surface, in strumous ophthalmia, I find them, in a majority of cases, decidedly injurious, and, under these circumstances, they seem very much to retard the cure, and to exert a baneful influence for some time after they have been discontinued. The presence of photophobia, however, does not alter the rules I have already laid down for your guidance, in a former lecture, with regard to the employment of such means. If the secretion is of a puro-mucous character, stimuli will be of service, but they will not subdue the disease single-handed, as in pure catarrhal ophthalmia, but must be combined with other suitable means, both constitutional and local, and if the discharge becomes aqueous in character, the local treatment must be altered. Cases do occasionally occur in which the symptoms are severe and protracted, and in which I have observed, both in my own practice and in that of others, great benefit from the use of nitrate of silver, although none of the conditions above-mentioned, as indicating this remedy, were present. My present motive for its employment, under such circumstances, has been the exhaustion of all other available means, and rather as a *dernier ressort* than as the result of any scientific induction. Such occasional and accidental exceptions to a rule do not at present advance our knowledge of treatment, but ought in candor to be mentioned, and may ultimately lead to the elucidation of a fresh law. In those forms of strumous ophthalmia, in which irritability is a prominent symptom, in which the disease is limited to the conjunctiva, forming aphthæ and phlyctenulæ, attended with aqueous secretion of a frothy character, local sedatives are very useful. Of these, perhaps the best is the opium wine of the Pharmacopœia, dropped into the eye two or three times a day; it generally causes rather severe pain, that lasts several minutes, but is followed by a marked improvement in the symptoms. Prussic acid is highly extolled by some authorities; it may be used in the form of vapor, or as a lotion, or drops, diluted in the proportion of a drachm of prussic acid, of the Pharmacopœia strength, to an ounce of dis-

tilled water. I have certainly observed great advantage from its employment in several cases. I can speak with more confidence of the opium wine than of any other form of sedative as a local application; it is an old but very valuable remedy that has fallen into unmerited neglect. This has probably arisen from its too indiscriminate use, and from a want of care and judgment in selecting cases suitable for its employment.

It will be seen in the foregoing remarks that I have endeavored to map out some of the more salient phases under which this very complicated disease presents itself to our notice, and to indicate the treatment best suited for each form: but I feel, in common with all who have an opportunity of extensive observation, how extremely difficult it is to convey in a lecture an accurate idea of these distinctions, even when well marked, and how impossible it is by such an imperfect medium to indicate the various shades of difference that are constantly presenting themselves to our notice, and the slight gradations by which one form glides into and combines with another, necessitating constant modifications of treatment, and presenting many difficulties even to the most experienced. At the same time I am not without hope that the distinctions I have endeavored to draw may be found of practical value as starting-points for individual observation—may prevent any flagrant error in treatment, and may explain how it is that so many different and even antagonistic systems have found advocates, and have been productive of good results. After all, it must be admitted that cases sometimes present themselves in which the treatment must be tentative; you must administer such varied means as have been found useful in the succession of their probable efficiency, remembering that many of the worst forms of this disease depend upon a state of constitution over which we possess but little power, and therefore that they must be necessarily protracted. In conclusion, let me remind you that there is a gradual curative tendency in all such cases, and hence much care is needed not to thwart these spontaneous efforts by too active or deplatory a mode of treatment, the effect of which must be to undermine those powers, and paralyse those efforts, by means of which Nature will gradually effect a cure.—*George Critchett, London Lancet.*

Phthisis Pulmonalis.—The following article we transfer to our pages from the *New York Medical Times:*

Gentlemen.—In reference to the employment of *Iodide of Ethyle* in the treatment of Phthisis Pulmonalis in your March number, I may state in continuation of the subject, the leading features of four well-marked cases of phthisis in which I have

employed it. I have also given it in several other cases: in one, it had to be abandoned owing to the production of anæsthesia by very small doses; in some, the phthisical symptoms were not marked with sufficient distinctness to afford evidence of its value; and in others, it was used so irregularly, or for such a short time, that no definite conclusion could be drawn regarding its effects; and I may mention, in passing, that two circumstances militate very strongly against the regularity of its employment by patients; some concluding from the smallness of the dose, and its simple mode of administration, that its influence could be but trifling, while others are deterred from its use by a suspicion of the whole class of inhaling remedies—an amount of caution which is unfortunately too well grounded, owing to the manner in which this mode of treatment has been paraded before the public. The first case I shall mention is that of H. P., a young man aged 19, who commenced the use of the ethyle on the first of December, 1854, at which time he presented the following symptoms: Had suffered from cough for eight months, the cough being attended latterly with muco-purulent expectoration; the sputa were frequently streaked with blood, and once about half an ounce of what was described as being unmixed arterial blood, was raised; there was some dyspnœa, and considerable pleurodynic pain after exercise; nocturnal hectic was of tolerably frequent occurrence; and there was general lassitude and debility. There was marked dullness on percussion, anteriorly, on the right side of the thorax, extending from the second to the fourth intercostal spaces, where also there was obscuration or obliteration of the natural, respiratory murmur; moist, mucous râle and ronchus were audible; and there was pretty distinct broncophony. In other portions of the right side, and over the left side of the thorax, the respiration was exaggerated and puerile, but otherwise presented no indications of disease. He had taken cod-liver oil, and employed counter irritation over the affected lung, without much amelioration of the thoracic symptoms, but he had gained somewhat in weight. He now commenced inhaling the iodide of ethyle, using fifteen minims three times a day, and continued it regularly till the 20th of Feb., 1855, when it became necessary to suspend its use, owing to diarrhœa, frontal headache, nausea, and other symptoms of iodism having made their appearance. Previous to these symptoms, and after their subsidence, which took place shortly on the suspension of the remedy, his condition was and has been most satisfactory; he has scarcely ever coughed, there has been no return of hectic or hæmoptysis, and he has been gaining flesh and strength rapidly. The chest, on percussion over the affected portion, exhibits but very slight dullness, and the respiratory murmur approaches very nearly a natural character.

In another case which presented the symptoms of tubercle in its earliest stage, accompanied with very severe bronchitis, the use of the ethyle for a few days, combined with counter-irritation, confinement, and a strict antiphlogistic regimen, sufficed to effect the complete suppression of the cough and dyspnœa, which, as yet, after an interval of some months, have not reappeared.

The other two cases in which I have had an opportunity of testing its utility with regularity, presented unmistakable evidence of the existence of a small cavity immediately under the clavicle, in the one case occupying the right, in the other the left lung. In both of these cases the severity of the symptoms has been much mitigated, and the progress of the disease arrested; and I believe it to be possible that a cicatrizing process may yet be established.

It will be urged that equally good results in cases of phthisis have been secured by the use of counter-irritation, the administration of cod-liver oil, and strict attention to the hygienic and dietetic rules which form what may be called the routine treatment of the disease; and this I think no one will be inclined to deny; but, at the same time, I am convinced that the iodide of ethyle will be found, in most cases, a most valuable adjuvant, and that the practical results will be found fully to bear out the theoretical conclusion drawn from a consideration of its chemical composition. Its action may take place in two ways: it must, in the first instance, exert a direct sedative local action on the diseased part; and secondly, from absorption, a general or constitutional action; and it is not unreasonable to suppose that it exerts an influence in the early stages of tubercular diseases by promoting absorption, and later, by aiding cicatrization.

I do not doubt that the real and primary cause of phthisis is still to be sought for in defective action of the organs of assimilation; and also that its ultimate cure, if ever, is to be found in the restoration of the healthy discharge of these functions; and that it is to the attainment of this object that our efforts should be mainly directed. This must of necessity be a work of time; and I believe that the iodide of ethyle while aiding the measures which we employ for this purpose by its general alterative and tonic action, is also, by its local action on the diseased part, gaining us the time necessary for the full development of our plans.

The expensive nature of the substance, and the difficulty of procuring it in sufficient purity, form at present a serious barrier to its employment, and have induced some, I believe, to use in its stead a mechanical solution of iodine in sulphuric ether. If the employment of this mixture is attended with any benefit it must be owing entirely to the sedative action of the ether, as very little if any of the iodine will become volatilized; a fact of which any one may easily convince himself by placing a portion of the solution in a watch glass, evaporating dish, or other vessel,

previously weighed, permitting evaporation to take place, when the sides of the vessel will be found coated with iodine and it will have gained in weight in exact proportion to the quantity previously held in solution by the ether. I do not know of any indications which forbid a trial of the iodide of ethyle unless it be in those rare cases where very small doses produce anæsthesia. It has been given with great advantage in many cases of chronic bronchitis, and also by the mouth in some cases of rheumatism and other diseases where iodine is an appropriate remedy. It is not improbable that others of the compounds of the organic radicles are endowed with equally valuable medicinal properties; and the subject presents a wide and interesting field for therapeutical investigation.

52 *Amity Street, N. Y.* HENRY FISHER, M. D.

On Sterility depending on certain diseased states of the Lining Membrane of the Womb: its treatment and cure. By Wm. Cumming, M.D., F.R.C.P., Edinburg, Vice-President of the Edinburg Obstetrical Society.

Cases of essential and incurable sterility depending on the female are extremely rare; and it is not my purpose to refer to the cause of this form. But cases of removable sterility are very numerous; and it may be interesting to detail some of the causes of it, the treatment, and the results.

I. One of these causes is a diseased state of a portion of the lining membrane of the uterus in cases of mal-position, the diseased part corresponding with the angle formed by the flexion of the womb on itself. According to my observation, the displacement of the womb most frequently accompanying this morbid condition of the mucous membrane, is anteversion; but other forms of displacement are not exempt from disease. I am now disposed to believe that no *mere* displacement or contortion of the uterus will prevent impregnation, and that it is only when this is accompanied by a congested or ulcerated or otherwise diseased state of the lining membrane that it is a cause of sterility; and the reason of this seems to me to be that the morbid part so flexed acts as a valve, which, while it allows a passage, painful or painless as may be, to the menstrual and muco-purulent discharges from within, refuses entrance to any fluid, such as the seminal, from without.

When these cases first came under my notice many years ago, being at that time inclined to attach more importance to mere displacement of the uterus than I now do, I attempted their cure by the means that were in use for the removal of the displacement. The chief means employed was the use of the intra-uter-

ine pessary, on the supposition that there was no disease of the womb; but the success of this by no means corresponded to my expectations. It was evident that there was more than mere displacement, and that recourse must be had to other means; and having discovered in some (not certainly in all) a preternatural degree of tenderness and induration at the point of flexion, I was led to the conclusion that the mischief lay there, and that the treatment should be directed to that part. Accordingly, instead of inserting the pessary, I introduced bougies of different sizes till the constriction that existed at the angle of the displaced womb was removed, and followed this up by applying the solid nitrate of silver to the congested or otherwise diseased surface. The result of this was good. The painful menstruation was often removed, always relieved, a more free menstrual discharge followed, the intra uterine leucorrhœa was by successive applications cured, and the patient in due time became pregnant. To this, of course, was added such treatment of the general system to be required.

Of this class the following may be regarded as a not uncommon specimen:

Mrs. Z., married three years, had before marriage been more or less out of health at the menstrual period, but after that event, had had her uterine symptoms much aggravated. She had for the two years previous to consulting me been treated by leeching, counter-irritation, etc., but without effect. She had, also, on one occasion, had a bougie introduced within the os uteri, but the pain caused was so exquisite that the lady fainted, and the operation was not repeated. When she came under my care, I ascertained that there was very decided anteversion, great tenderness at the curve of the uterus, i. e., at the part where it was anteverted on itself; and when I introduced a very small bougie for the purpose of examining the os internum, there was great tenderness and clearly some constriction. The leucorrhœal discharge was not very considerable, but it was *intra-uterine*, and irritated, and almost excoriated the vagina; and it was largest in quantity about midway between the menstrual periods.

It appeared to me that both the lady's illness and consequent sterility depended on the narrowness of the os internum, and probably also on a diseased state of the mucous membrane near it. There was no congestion either of the cervix or body of the womb, nor could I detect any other functional or organic derangement.

Having explained to the patient the nature of her case, and assured her that I could not undertake the treatment unless I was allowed to treat her by dilatation, to which, from her previous experience of the bougie, she had great objection, and having reduced considerably the tenderness of the diseased part by the

inunction with belladonna ointment before introducing the bougie, I succeeded in dilating the os internum, and ultimately in applying the solid nitrate of silver. The effect of this was soon perceptible. She had much less painful menstruation, more of the discharge, and of a more natural character, and the examination of the affected part by the finger was much less painful. This was repeated from time to time for three months, when she left Edingburg for her own home. About a year after she returned, complaining that she was not yet cured, and proposed a consultation with another practitioner, who, after a careful digital examination, recommended incision of the os. To this she would not consent, and perhaps fortunately, for she was then nearly a month pregnant, and in due time was delivered of a very fine child, since which her health has been good, and her local symptoms have disappeared. I may add that the last time she was in Edinburg—I mean at the time when she was a month pregnant—the anteversion was as considerable as it had ever been; and except that she voided urine more frequently than natural, I am not aware that she suffered in any degree from the displacement.

This is a fair sample of a large number of cases, in which the treatment is neither severe nor protracted, and the result is very successful. The probability is that the displacement and diseased condition of the mucous membrane have existed long before marriage, but have been aggravated by it. In such cases, so far as my experience goes, dilatation by bougies, and the application of the solid nitrate, effect a cure, and are not liable to produce any dangerous or severe symptoms. In this respect the latter is much preferable to an injection of the solution, not a few disastrous results having followed the escape of the injection into the peritoneal cavity.

II. Another class of cases occurs similar to that now reported, but without displacement of the womb. [By this term I mean flexion of the womb on itself, of so decided a character, that turn or twist it how you may with the uterine bougie it always reverts to the same mal-position, which is certainly not the case with many of what are called dislocations of the womb.]

The essential nature of this class appears to me to consist in marked constriction of the os internum, with ulceration of the lining membrane above the constriction and this ulcer often accompanied with induration of its base, and of part of the neighboring tissue. Whether the constriction precedes the ulceration or the reverse I do not pretend to say; but I have no dobut that the ulcer increases the constriction, and that the removal of the former is essential to cure. For this purpose I invariably dilate the os externum and internum and the cavity of the cervix, and apply the solid nitrate of silver very freely to the ulcerated or diseased surface, and with the best results, by which I mean

removal of the local and general symptoms complained of by the patient, and, in time, of the sterility—I say in time, for in most cases impregnation does not occur for some time after the apparent cure.

I may mention that this constricted and ulcerated state of the lower part of the uterus produces two effects, which are calculated to mislead, and do very often mislead, practioners. It induces a hypertrophied condition of the body, and a considerable enlargement of the cavity of the uterus; and till I was satisfied of this by a (comparatively) frequent occurrence of such cases, I was inclined to regard, and did in reality often regard them as cases of hypertrophy, and so employed a treatment that not only failed of its anticipated effect, but weakened the patient, and greatly increased the local symptoms as well as rduced the general health. I am quite confident that no amount of depletion, either by leeches or scarification, and that no local application of ointment will remove the constriction and ulceration, though they may for a time relieve the congestion, heat, and irritation that generally accompany them, but which soon disappear without weakening the treatment when their cause has been removed; and while I cannot help insisting that the repeated application of leeches in the treatment of uterine disease is very rarely necessary, I cannot help also declaring that I have known many cases in which the health of the patient has been seriously impaired, and life even compromised, by such treatment.

Any practitioner who has seen much of uterine disease may verify what I have said in regard to ulceration *within the womb*, by what he obsarves through the speculum in those cases where the ulceration is on the *vaginal portion of the os externum*. A patient with an anxious, weary expression of countenance, and complaining of the ordinary local and general symptoms of leucorrhœa from ulceration, comes to consult us. On examination, we find the expected ulceration or abrasion, and some congestion. We apply the solid nitrate from time to time till the ulcer cicatrizes. We do not deplete or mercurialize—in short, we do not weaken the patient. She recovers her health, her anxious expression vanishes, and in course of no long time she becomes pregnant. This, which we *do see*, as occurring at the *os externum*, occurs still more frequently at the *os internum*, where we *cannot see* it, and precisely the same treatment is applicable to the one as to the other, with this addition, that before we can apply the nitrate of silver in the latter case we must dilate, and with this difference, that while the former or external species of ulceration almost invariably occurs in those who have had one or more children, the latter or internal, almost as invariably is found in virgins and in those who have had none.

The following is one of many illustrating this form of the disease:

Mrs. A. has been married for nine months, when she had what was supposed to be a miscarriage; but from the menstruation, though for several months very scanty, having never been entirely suspended, and from a minute examination of the os and cervix uteri, I was quite satisfied that she had never been pregnant. When she first consulted me, she believed herself again at about the end of the third month of pregnancy; and as I was unwilling to incur the charge of having induced abortion, I contented myself with making a superficial examination, and waiting till time should put it beyond question one way or the other. She had however been regularly, though very scanty, menstruating, and though after a time she increased in size, and her mamma (*not* the areola) enlarged and she had morning sickness, and many other of the signs and symptoms of pregnancy, I was quite confident that she was not pregnant. Still, she and her friends deprecated interference, and she went on till she reached the (supposed) 7th month. At this time I was desirous to begin the treatment that I thought likely to remove both the disease and the sterility, and requested the opinion of a professional friend, who agreed with me as to the non-pregnancy. I then examined with the bougie, and found marked constriction at the os internum, and *very acute pain*, produced by the passage of the bougie—pain described as being similar to that produced by the extrusion of a small clot of blood during the menstrual period. The leucorrhœa was intra-uterine, and in considerable quantity about midway between the two periods.

The treatment (local) consisted in dilatation of the passage as far as the cavity of the body of the womb; and in effecting this I remarked what is, I believe, very common in such cases, that after passing the constriction at the internal os the bougie reaches a cavity of much larger dimensions than natural, in which it can be moved about with freedom, and yet containing no polypus or tumor, and with its walls slightly increased in thickness, as if the effort to expel the clots at the menstrual period through the narrow neck had given rise to this form of hypertrophy with dilatation, as is seen in the case of the heart. The dilatation was followed by the application of the solid nitrate of silver to the diseased part, and this was repeated till all tenderness was gone. Complete relief from pain during the menstrual period was the consequence, and ultimately the patient became pregnant. She has since her delivery, which took place at the full time, enjoyed perfect health.

It were easy to detail many such cases, but they are all more or less alike.

III. Another cause of sterility is a diseased state of the lining membrane of the cavity of the uterus, not necessarily, though not unfrequently, accompanied by the constricted and ulcerated state of the cervix referred to in the preceding section.

The chief symtom of this is the persistent continuance of uterine leucorrhœa in very considerable quantity, attended by the usual weakness, discomfort, irritability, and despondency, observed in most affections of the womb. The patient feels better at and immediately before and after the menstrual period, but feels all her ills heavy on her in the intermediate time.

In these cases the whole or greater part of the lining membrane of the womb is diseased. It is quite possible that the seminal fluid may pass into the womb, so as to come in contact with the ovum on its way from the ovary; but it is probable that the ovum (impregnated) when it reaches the womb does not find a healthy point of contact, and that therefore it passes through and perishes. In short, there is frequent impregnation, and as frequent destruction of the ovum. The object in view, therefore, is to restore a healthy state of the mucous membrane, and thus at the same time remove the disease and the sterility. The process of treatment is similar to that for the previous class of cases, with this difference, that the cavity of the body of the womb requires to be cauterized. This should be done at the end of the first week after the menstrual period, and repeated once a month till a healthy state and action are induced.

It is possible that the treatment of this class of cases may be conducted on different principles with success; but to me the plan mentioned seems the most simple, direct and successful, and it has this great recommendation, that it is quite as safe to apply the caustic to the inside as to the outside of the womb. I have said nothing of the general treatment; but though very important there is nothing in it very different from what has been long and is every day pursued.

Mrs. M. had been married for several years, and had enjoyed good health till her marriage. From that event she dated her complaints, all of which pointed to the uterine system. In this case the prominent symptom was the uterine leucorrhœa between the menstrual periods, commencing a few days after the disappearance of the menstrual fluid, and continuing generally for ten days. She was comparatively well just before and after the menses, but when the white discharge appeared she *felt* there was something wrong, something that weakened and reduced her; and though she had undergone treatment of various kinds for it, the disease still persisted. On examination I could detect lateral displacement of the uterus, but no constriction. The body of the womb, however, was tender to the touch, and the bougie, when fully introduced, occasioned unusual pain. It appeared to me that

the disease in this case lay in the cavity of the body; that the lining membrane was affected over a considerable surface; and that the treatment should consist in very free and repeated constication of the whole mucous membrane. Having dilated the cervix with a sponge tent, I did so, and with a very gratifying result. The body of the womb became gradually less tender, the leucorrhœal discharge diminished, and lost its muco-purulent character; the vagina, which was tender from the acrid character of the discharge, became smooth and soft; the back lost its weakness; the general health became restored; and ultimately pregnancy supervened with a favorable result.

In conclusion, I may add that in connection with this mode of treatment, there is a class of miscarriages in which this cauterization of the internal surface of the uterus is very successful—I mean those in which an abortion has occurred between the second and third month, followed by general weakness, from which the patient does not recover, and the other uterine symptoms already detailed, and where a second pregnancy seems impossible. In these cases examination with the uterine bougie generally communicates the feeling of its coming in contact with a rough, somewhat hard, uneven surface.—*London Lancet.*

Pneumonia.—Prof. Flint made to the Buffalo Medical Association, in September, an elaborate and highly interesting report on the pathology, prognosis and treatment of Pneumonia, from which we make the following extract:

In regard to the effect of blood-letting on the duration of pneumonia, the papers referred to the committee, communicated to the association by a member of the committee, contain certain facts which are to be noticed. Four cases of pneumonia are reported in which blood-letting was employed in all on the fourth day; in two of the cases being practiced three times, and in two once only. In one of the cases the patient was a child 4½ years of age. The duration of the disease in these cases, respectively, are are given as follows: 8, 5, 4, 2½ days. The mean duration is about 8¾ days. This is a small fraction below the mean duration in Dr. Ames's fifty-one cases treated with neither bleeding, antimony, nor mercury. The cases reported by a member of this committee, Dr. Burwell, were not given as illustrating a mode of practice to be pursued indiscriminately, or even generally, in the treatment of pneumonia; but as evidencing the success following blood-letting under circumstances which appeared to him to indicate in a special manner the employment of this measure. I shall not discuss the propriety of the practice pursued in these cases. Considering

their terminatioon and duration, the only question for discussion is, whether the detraction of blood was necessary; in other words, whether the favorable progress of the disease was not due to its natural tendencies irrespective of the treatment. This is a question more easily discussed than answered.

In connection, however, with the results in the cases reported by my associate on the committee, I will give the duration in six cases occurring during the present summer, and treated by me, with the exception of one case which I saw in consultation with Dr. Samo, but observed, and of which I took notes during the whole career of the disease. Five of these cases were male patients at the Hospital of the Sisters of Charity, and in the remaining single case the patient was a child, five years of age, treated in private practice. These are all the cases which I have observed during the present summer, in which blood-letting had not been practiced prior to their coming under my observation. In the case of the child a little antimony was prescribed. In this case the pneumonia was lobar, and uncomplicated with bronchitis. In all the other cases the treatment consisted of anodynes and stimulants. All the cases are fully recorded. In three of the cases an entire lung was consolidated. In the three remaining cases the inflammation was limited to the lower lobe. The cases came under observation at periods from the date of the attack as follows: 5, 3, 8, 8, 6, 1 days. Reckoning from the initial chill, to a period when the pulse had fallen to the natural standard, or below it, which not infrequently occurs, and the respirations but little, if at all accelerated, the duration in the cases respectively, was as follows: 11, 7, 14, 12, 7, 5. The mean duration is precisely 9½ days. The average duration exceeds that in the cases reported by Dr. Burwell, by a fraction less than a day. It is to be borne in mind that my cases were, with one exception, hospital cases; that the patients entered at a period from the date of attack, varying from three to eight days, had received no treatment prior to their admission, and in one-half the numbhr of cases the inflammation extended over the entire lung on one side. In each of the cases the recovery was rapid. The duration to the time of being dressed, in the cases severally, is as follows: 14, 9, 20, 21, 8, 6 days. In three cases the patients remained in hospital for some time after being able to be discharged, two of them being employed as out-door assistants. The remaining two patients were discharged each on the fifteenth day from the date of the attack. Medical attendance on the child was discontinued on the eighth day.

The few cases reported by Dr. Burwell and myself, thus present a mean duration not far from equal; the former treated with and the latter without bleeding. In neither collection is the number of cases sufficiently large to warrant statistical deductions; but

they serve, exhibited in contrast, to exemplify the general conclusion at which we must arrive after a consideration, be it ever so elaborate, of the evidence for or against the employment of blood-letting in pneumonia, namely, the propriety of this measure of therapeutics must be determined by the practitioner in each case separately, not by following any fixed formula; but by a rational interpretation of the indications which cases individually offer, guided by certain general principles regulating the employment of blood-letting in all acute inflammations. This conclusion embraces postulates, which to unfold and treat of folly would require more space than falls within the proper limits of this report. Assuming that we should be led to it had we time to follow the chain of considerations by which it is reached, diversity of practice among practitioners hinges mainly on the different views entertained respecting the general principles regulating the employment of blood-letting in all acute inflammations. Here, again, is a broad field of inquiry involved in a full investigation of the present subject. It will not, of course, be expected that I should enter upon it; but it *will* probably be expected that I shall not leave the subject without farther expression of my views respecting the treatment of pneumonia by blood-letting. My remarks already occupy so much space that I ought not to presume on your attention by extending much farther this report; and I shall, in conclusion, for the sake of brevity, embrace in a series of propositions certain practical points of consideration, which, if my leisure and your interest permitted, I should present in a shape not to be disposed of so cursorily. As it is I can only add a very few brief comments, leaving a fuller exposition for oral discussion.

1. Primary pneumonia in the form in which it is usually presented in the adult, namely, confined to the lower lobe of one lung, tends so uniformly to recovery, that interference by blood-letting is not requisite as a means of securing a favorable issue of the disease. The objects, then, for which blood-letting is practiced in such cases must be embraced in the following enumeration: *a.* To arrest the disease. *b.* To abridge its duration. *c.* To limit the diffusion of the inflammation. *d.* To lessen the local morbid effects incident to pulmonary inflammation. *e.* To relieve pain and other distressing symptoms. *f.* To promote rapid convalescence and the complete restoration to health.

2. It appears to be fully established that blood-letting does not possess the power of arresting the disease. Its employment with a view to this object is therefore unwarrantable, and, if not otherwise indicated, must do harm in proportion to the want of ability on the part of the patient to bear up under the effects of so potent a therapeutical measure.

3. Clinical facts appear to show that in a series of cases blood-

letting exerts a limited influence in shortening the duration of pneumonia. We have not, however, evidence that this influence is exerted save in a ceriain proportion of cases, and it by no means authorizes the employment of blood-letting in cases indiscriminately. The effect on the duration of the disease is to be regarded as incidental to its judicious employment under other indications. To bleed every patient because it has been found that in a large collection of cases the average duration is less in the cases bled than in those not bled, is a vicious application of statistical results. To abridge the duration of the disease, is, therefore, not a legitimate object of taking blood, disconnected from other objects.

4. The extension of inflammation from one lobe to another in the same side, or in the opposite side, which occurs in a certain proportion of cases, adds to the gravity of the disease, and it is, of course, desirable to prevent its occurrence. We have not, however, positive evidence that blood-letting possesses any power to limit the diffusion of the inflammation. There is reason to believe that when the inflammation subsequently invades other lobes than the one primarily attacked, it is due to an internal tendency which, with our present knowledge and resources, we are unable to control. It is probable that instances in which an entire lung is affected, or in which the pneumonia is double, would be found not less numerous among a certain number of patients bled, than among the same number not bled. This is an interesting point to be settled by statistical researches.*

5. The local morbid changes incident to inflammation, namely, inflammatory exudation leading to obstruction of the air resides and interruption of the circulation through the parts inflamed, occur very uniformly in pneumonia, and, generally, early in the disease. It must be considered doubtful whether, to much extent, these changes may be lessened by blood-letting. The problem is a difficult one to determine, on the one hand, in cases in which blood-letting is practiced, whether the local changes are less than they would have been had no blood been taken; and, on the other hand, in cases in which blood-letting is not practiced, whether the changes are greater than they would have been had this measure been employed. Considering that the physical signs of solidification are generally present in cases bled, as well as in those in which blood-letting is not practiced, the effect of the measure in limiting the amount of exudation is probably small, and exerted in only a certain proportion of cases. While we are not warranted in denying that blood-letting ever lessens the local changes in pneumonia, the grounds for expecting this result are not suffi-

* Whether this point has been made the subject of statistical investigation, or whether data for such an investigation are available, the writer, at the moment of preparing this report, is unable to state.

cient to constitute it an object for taking blood under circumstances when, if this effect be not produced, the condition of the patient will be damaged. It is obvious that with reference to this object any efficiency which blood-letting possesses depends on its employment within the first two, three or four days.

6. To relieve pain and other distressing symptoms is a legitimate object of therapeutics, and that blood-letting in certain cases is followed by a marked abatement of the local symptoms cannot be doubted. In resorting to paliative measures, however, their ulterior effects are not to be overlooked. It would manifestly be bad practice to incur a risk of doing harm remotely, in order to secure a temporary alleviation of pain or distressing symptoms. Blood-letting, then, may be practiced early in pneumonia with a view to relieve the acute pain, and labored respiration present in some cases, but not under circumstances contra-indicating it, and not carried to an extent which will be likely to involve ulterior damage.

7. The objects to be attained by blood-letting, it is evident, relate to the existing disease. No one, it is to be presumed, advocates taking blood without reference to present indications or the immediate effects, but with a view to its influence on convalescence and the completeness of the recovery. On the other hand, the influence which the loss of blood may have in retarding convalescence, and leaving the system more enfeebled than it would have been from the effects of the disease alone, should restrain the practitioner from employing blood-letting in some instances, when, without any regard to its remote consequences, it might be indicated. The evils of unnecessary and excessive blood-letting in the disease are probably manifested by slow and incomplete restoration to health, rather than by an increased rate of mortality.

8. In view of the considerations contained in the preceding propositions, the objects for which blood-letting is practiced in pneumonia are few and limited; and in resorting to it the practitioner must be guided by circumstances pertaining to cases individually, and by the general principles regulating its application to acute inflammation. These circumstances and principles will supply the subjects of the few remaining propositions.

9. The circumstances indicating blood-letting in pneumonia, as the rule, belong only to an early period of the disease. Indications are rarely presented after solidfication has occurred. The processes of restoration depending on the energies of the system are not likely to be favorably affected by the loss of blood; but, on the contrary, the recuperative power may be diminished.

10. General indications for blood-letting in acute inflammations have relation to circumstances incidental to the inflammation rather than to the inflammation itself. They relate to the degree and character of the febrile movement; to the previous condition

of the system; to the habits and constitution of the patient. Special indications in pneumonia relate to pain and embarrassment of respiration.

11. In pneumonia, as in acute inflammations generally, blood-letting is indicated, as the rule, only when symptomatic fever is present, and of a high grade, as denoted by increased frequency, and force of the pulse, the symptoms showing not simply morbid action, but augmented power of the circulation.

12. Polyæmia or plethora, previous good health, and vigor of constitution, are circumstances rendering blood-letting safe and proper; when, *per contra*, anæmia, feebleness of constitution, or impaired health from former disease, deprivations, over exertion, intemperance, etc., etc., in connection with the same degree and extent of local inflammation, render it inappropriate and perhaps unsafe.

13. In conclusion, blood-letting in pneumonia, as in other acute inflammations, in some instances not only conduces to immediate relief, but abates by an indirect action the intensity of the inflammation, and exerts a favorable influence on the progress of the disease. But in this, as in other affections, indiscriminately employed, or carried to an injudicious extent, it is a measure potent for harm, and may prove destructive. Employed in a manner to secure its benefits and avoid its evils, it is a useful remedy in a certain proportion of cases; and this proportion would be found to be not uniform in patients of different conditions; in the residents of a city and the country; in different countries and in different portions of the same country, and in successive years. Of cases occurring at the same time and in the same locality, in some it will be useful, in others hurtful. A just appreciation of the principles and rules which should regulate its employment, is essential; but in their application to individual cases, the line of practical conduct cannot be formularized, but must be left to the judgment, and perhaps it may be said the sagacity, which, in addition to scientific attainment, are necessary to form the skillful physician.

It will be observed that the remarks contained in the foregoing propositions have reference to primary, uncomplicated pneumonia in the adult. The applicability of blood-letting, as a general remark, is less when the disease is secondary, or complicated, or occurring at either of the extremes of life.—*Buffalo Med. Jour.*